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DEPARTMENT OF COMMERCE
BUREAU OF FISHERIES

PROGRESS IN BIOLOGICAL INQUIRIES, 1928

INCLUDING EXTRACTS FROM THE PROCEEDINGS OF THE DIVISIONAL CONFERENCE JANUARY 2 to 5, 1929

By ELMER HIGGINS

FISHERIES DOCUMENT No. 1068

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U. S. DEPARTMENT OF COMMERCE
R. P. LAMONT, Secretary
BUREAU OF FISHERIES
HENRY O'MALLEY, Commissioner

Bureau of Fisheries Document No. 1068

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APPENDIX X TO REPORT OF COMMISSIONER OF FISHERIES
FOR THE FISCAL YEAR 1929

UNITED STATES
GOVERNMENT PRINTING OFFICE
WASHINGTON : 1930

For sale by the Superintendent of Documents, Washington, D. C. - - - - Price 20 cents

PROGRESS IN BIOLOGICAL INQUIRIES, 1928 [1]

INCLUDING EXTRACTS FROM THE PROCEEDINGS OF THE DIVISIONAL CONFERENCE, JANUARY 2 TO 5, 1929

By Elmer Higgins

Assistant in Charge of Scientific Inquiry

(With the collaboration of investigators)

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[1] Appendix X to the Report of the United States Commissioner of Fisheries for 1929. B. F. Doc. 1068.

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Part I.—INVESTIGATIONS CONDUCTED DURING 1928

INTRODUCTION

The division of scientific inquiry of the United States Bureau of Fisheries is still responsible for the discharge of duties that formerly comprised the entire activities of the original Fish Commission. At the time of its founding the commission was required to prosecute the inquiries necessary to ascertain whether and what diminution in the number of food fishes of the United States had occurred, the causes of any decline observed, and what measures should be adopted for the protection of the declining fisheries. The scope of the bureau's activities has since been increased by the addition of divisions responsible for the propagation and distribution of food and game fishes, for investigations in technology and the collection of economic statistics of the fisheries, and for the administration of the fisheries and seal industry of Alaska. The division of inquiry, however, prosecutes scientific investigations for the purpose of supplying information necessary for wise conservation. Fishery administrators, State legislatures, commissions, and the fishery industries at large are furnished, in so far as our knowledge permits, with necessary information for the regulation of the fisheries and for the guidance of private industry in wise and safe exploitation of the Nation's aquatic food resources.

The division is, therefore, an investigative and advisory body having no administrative authority over any of the fisheries of America. Its investigations are designed with two definite aims—(1) to provide information for immediate use and (2) to contribute to the upbuilding of a coherent body of knowledge which is rapidly assuming rank among the various fields of biology as fishery science. The bureau is continually confronted with a host of practical and urgent questions regarding the fisheries: What becomes of the butterfish when it leaves the inshore waters in the wintertime? How long can the haddock fishery continue to increase on South Channel grounds? What are the probabilities of continued heavy catches in the mackerel fisheries? What can be done to prevent the greening of oysters in Connecticut? Should the inshore waters of the Texas coast be closed to commercial fishing to protect the fish supply? How great must be the spawning escapement of salmon

in various rivers of Alaska to produce abundant runs in the future? All of these are intensely practical and many of them are urgent questions requiring early solution. In addition to these inquiries the division endeavors to satisfy private citizens who propound questions of amazing variety, ranging from requests for assistance in private frog culture to furthering plans for the international regulation of whaling. The bureau's scientific work, however, is so organized that in providing answers to these questions sound contributions having a general significance to fishery science are made.

The demands for investigations in all sections of the country continue to exceed the facilities of the bureau, and it is therefore necessary that the coherent program that has been developing for many years be followed with scrupulous care. The three main phases of this program, comprising investigations pertaining to conservation of the commercial fisheries, studies of aquiculture or the propagation and cultivation of fresh-water fishes, and the development of shellfish culture, by no means cover the entire field of fishery research. Well-conceived plans have been made, however, and presented to Congress to extend investigations in these respective fields during the next 5 years to all of the major fisheries of the country and to the important areas in the interior.

Outstanding results during 1928 have been obtained in the investigation of the fisheries of the Pacific coast, where the salmon fisheries, of Alaska in particular, are being brought under more perfect scientific regulation and control than perhaps any other great fishery in the world. Equally gratifying results are being obtained in understanding the causes of variations of yield in such apparently erratic sea fishes as the mackerel, and a substantial groundwork is being laid to solve the problems of supply in the shore fisheries of the middle-Atlantic section.

In the field of aquiculture refinements have been made in the methods of increasing production in ponds and inland water areas of the Mississippi River Valley. Pond-culture studies have been unusually successful. Production of such important food and game fishes as the basses has been increased by proper methods of handling to twice former normal production, and many of the factors that determine production in ponds have been ascertained by painstaking study.

A forward step of great importance has been taken in Great Lakes fishery conservation in the adoption of adequate fishery statistics by a number of States bordering the Great Lakes. The bureau's investigators have been active in urging the adoption of a statistical system that is uniform throughout the Great Lakes region in order that yields may be compared by districts and in various years with sufficient exactness to afford an index of abundance that will give evidence of depletion at the earliest moment and serve in determining the effectiveness of regulations on the fishery that have been and may be imposed. Such a system, which approaches the one in use in California in detail and accuracy but differs from it in method, is being adopted by various States and will soon cover the entire region.

One important new project was undertaken during 1928. This was the study of means of preventing the loss of migrating fish in the streams of the Pacific northwest, where annually millions of young salmon are destroyed on their seaward migration by entering irrigation ditches and power diversion canals. Power dams also cause considerable loss to adult fish, which are unable to surmount such obstacles to their spawning migrations. An appropriation of $25,000 was provided for the purpose of devising suitable screens or diverters to prevent young fish from entering such irrigation canals and also to improve the design of fish ladders for use of adult migrants. A suitable type of fish screen has been selected, experimental installations have been made, and it is proposed to install such devices on all the Government reclamation projects.

The division has always been fortunate in securing the wholehearted and generous cooperation of various State and private agencies, thus materially increasing its investigations in both extent and effectiveness. During 1928 the States of Maryland, North Carolina, Georgia, Texas, Michigan, Ohio, Wisconsin, New York, Washington, Oregon, and California officially entered into such cooperation. In addition, formal or informal cooperation of the Museum of Comparative Zoology of Harvard University, the Bingham Oceanographical Collections of Yale University, the Pepper Laboratory of Clinical Medicine and the Zoology Department of the University of Pennsylvania, the Buffalo Museum of Science, the university museums of the University of Michigan, the University of Wisconsin, University of Missouri, Duke University, Leland Stanford Junior University, and the College of Fisheries, University of Washington, is gratefully acknowledged. In the Great Lakes fishery investigation a cooperating investigator from the fisheries department of the Province of Ontario also took part.

A single meeting of the North American Committee on Fishery Investigations, an international body including representatives of Canada, Newfoundland, and the United States, was held during 1928 at the University of Toronto, Toronto, Canada. Various matters pertaining to the collection of statistics of the cod and haddock fisheries in North Atlantic waters and to the proposed haddock investigations in both Canada and the United States were considered. A matter of perhaps the most pressing importance to Canada and to the United States, namely, the possible effect on the fisheries of the proposed damming of Passamaquoddy Bay, was given some extended consideration by the committee. For the guidance of the Canadian Government the committee was requested to appoint a technical commission from its membership to visit Passamaquoddy Bay, examine the conditions and consider the plans of the power company, study available information bearing upon the predictable effects of the project on the fisheries of the region, and report its findings to the Governments of the United States and Canada.

During the year 1928, 10 scientific or administrative reports were published under the supervision of the division. These papers originate in two ways. Some are the results of investigations supported entirely by funds of the division and are prepared by members of the scientific staff, while others are the results of researches on subjects of direct interest to the bureau, but which are conducted by private

investigators who receive little or no financial support from the division. The list of papers follows:

Progress in biological inquiries. By Elmer Higgins. 8°, 167 pp., 18 figs. Document No. 1029.

Annotated list of fishes collected in the vicinity of Greenwood, Miss., with descriptions of three new species. By S. F. Hildebrand and I. L. Towers. Royal 8°, 32 pp., 9 figs. Document No. 1027.

Biological survey of the upper Mississippi River, with special reference to pollution. By A. H. Wiebe. Royal 8°, 31 pp., 1 fig. Document No. 1028.

Production and distribution of cod eggs in Massachusetts Bay in 1924 and 1925. By Charles J. Fish. Royal 8°, 44 pp., 16 figs. Document No. 1032.

Fishes of Chesapeake Bay. By S. F. Hildebrand and William C. Schroeder. Royal 8°, 366 pp., 211 figs. Document No. 1024.

Quantitative study of the changes produced by acclimatization in the tolerance of high temperatures by fishes and amphibians. By Edward S. Hathaway. Royal 8°, 24 pp., 9 figs. Document No. 1030.

Nature and extent of fouling of ships bottoms. By J. Paul Visscher. Royal 8°, 60 pp., 40 figs. Document No. 1031.

Experimental study of the function of the oyster gills and its bearing on the problems of oyster culture and sanitary control of the oyster industry. By Paul S. Galtsoff. Royal 8°, 39 pp., 13 figs. Document No. 1035.

Food of bullheads. By Louella E. Cable. 8°, 15 pp., 2 figs. Document No. 1037.

Electric fish screen. By F. O. McMillan. Royal 8°, 32 pp., 21 figs. Document No. 1042.

The following progress reports covering the more important investigations conducted by the division during the calendar year 1928 were prepared in the main by the investigators in charge of various projects.

ATLANTIC AND GULF COAST FISHERY INVESTIGATIONS

LIFE HISTORIES AND MIGRATIONS OF COD, POLLOCK, AND HADDOCK

The study of the life histories of the cod, pollock, and haddock, by William C. Schroeder, which began in 1923, was in progress up to the end of 1928. Most of our efforts and results have been concerned with the cod, so that the study of migrations and the relationship between various stocks of fish living on different banks refers chiefly to this species. The haddock, however, is being sought after more and more each year by commercial fishermen, and in the future it must be given a full share of attention. A statistical summary of tagging operations conducted is given in the tables that follow.

Number of fish tagged

Item	1923	1924	1925	1926	1927	1928	Total
New York and New Jersey	0	0	0	0	167	550	717
Massachusetts, south of Cape Cod	10,231	4,384	6,143	1,730	6,123	1,247	29,858
Massachusetts, north of Cape Cod	13	163	314	0	68	87	645
New Hampshire and Maine	0	5,801	8,803	1,412	2,385	1,180	19,581
Georges Bank	0	0	0	1,103	726	173	2,002
Browns Bank and vicinity	0	0	0	0	1,559	554	2,113

Item	1923	1924	1925	1926	1927	1928	Total
Number of cruises	7	9	16	4	11	[1] 9	56
Days of actual fishing	43	51	76	23	64	[1] 68	325
Hours of actual fishing	333	320.5	461	167	428.7	(1)	
Cod tagged	7,618	6,209	10,420	3,565	8,851	2,834	39,497
Pollock tagged	2,215	916	949	39	413	267	4,799
Haddock tagged	411	3,223	3,891	641	1,764	690	10,620
Total number of fish tagged	10,244	10,348	15,260	4,245	11,028	3,791	54,916
Number of fish recaptured to Dec. 31, 1928	332	550	1,131	196	414	30	2,653

[1] During 1928 fish were tagged both by the *Albatross II* and by using the equipment of fishermen. The *Albatross II* made 7 tagging cruises, fished 292 hours on 51 days, and tagged 2,422 cod, 267 pollock, and 690 haddock.

In addition to the 2,653 fish with tags that were recaptured, there were 125 with "tag marks" taken by the vessels *Halcyon* and *Albatross II*. These latter fish had lost their tags but showed unmistakable evidence of having been marked. A summary of all fish recaptured follows:

Date of tagging	1923	1924	1925	1926	1927	1928	Total
Cod:							
1923	159	90	34	4	2	0	289
1924		206	250	37	5	0	498
1925			646	387	64	2	1,068
1926				52	153	9	214
1927					223	191	414
1928						25	25
	159	296	899	480	447	227	2,508
Pollock:							
1923	11	25	7	2	0	0	45
1924		4	21	1	0	0	26
1925			7	8	4	0	19
1926				0	3	0	3
1927					0	1	1
1928						0	0
	11	29	35	11	7	1	94
Haddock:							
1923	0	5	1	0	0	0	6
1924		14	39	2	3	0	58
1925			34	31	6	0	71
1926				4	12	1	17
1927					3	13	16
1928						5	5
	0	19	74	37	24	19	173
Total	170	344	1,008	528	478	247	2,775

Not included in the preceding tables are 946 cod tagged at the Bureau of Fisheries biological station at Woods Hole, Mass., in January, 1926, of which 23 have been reported recaptured; 422 cod tagged in January, 1927, of which 10 have been recaptured; and 491 tagged in January, 1928, of which 14 have been recaptured. These fish had been caught near by during November and were held until they had spawned.

In a report prepared one year ago it was estimated that about 70 per cent of the tagged fish lost their tags within the first year. On the basis of the past year's operations no change is made in this estimate. All fish had been tagged on the tail up to and including the summer of 1927, but in the fall of that year the tags were fas-

tened to the jaw. The recaptures during 1928 of these latter fish showed no improvement in numbers, so fish were again tagged on the tail.

During the early part of the investigation, experiments were made with various metals for tags, but monel has been in constant use since 1924. Tags of duralumin have been obtained and will probably be used in 1929 as soon as the present supply of monel tags is exhausted. The duralumin tags appear to have all the good qualities of monel but are much lighter in weight and may, therefore, prove superior.

The outstanding results of the fish-tagging experiments up to December, 1928, are as follows:

Cod grounds between southern Massachusetts and North Carolina.—Each fall, starting in October and continuing until the end of December, cod migrate from Nantucket Shoals and vicinity into the Rhode Island-Delaware region. Some go as far as Virginia and a few even to Cape Hatteras, N. C. The cod spend the winter in this region and return east in the spring (March and April), although a few of them may straggle back during the winter. During the summer cod are rarely caught west of Rhode Island.

Cod are found on Nantucket Shoals throughout the year, for not all of them join in this fall migration to the westward. In some summers, when the water on Nantucket Shoals is unusually warm (57° to 60° F.), part of the cod move 10 to 20 miles farther east into the Chatham Ground-South Channel region, where much cooler bottom temperatures prevail; but even in the warmest summers cod may be caught on Nantucket Shoals in commercial numbers.

During the summer, and sometimes from year to year, few immigrant cod reach Nantucket Shoals; but present records indicate that at times schools of cod migrate from Georges Bank to Nantucket Shoals. This migration appears to take place between late fall and early spring. A few of the Nantucket cod come from as far as Nova Scotia by way of the offshore banks, but only a straggler migrates south from the eastern coast of Maine.

Any one school of cod may be identified with the Nantucket Shoals region for from one to three years before it is caught up by the fishery or straggles away. Sometimes in the summer a large part of the stock of cod migrates toward South Channel and does not return. Few, however, migrate to the northward of Cape Cod, and only a straggler appears to pass Cape Ann.

New England coast from Massachusetts Bay to eastern Maine.—More cod migrate southward from the Massachusetts Bay region than migrate north. This region, however, supports relatively few cod. Farther east, along the coast of Maine, most of the cod are small, in the immediate shore waters being three years or less old. Most of them are caught up before migrating, but the greater part of those that do migrate go eastward to Nova Scotia.

In the middle of the Gulf of Maine, where there are a few isolated cod banks, such as Platts Bank and Cashes Ledge, cod may remain stationary from one year to the next or they may migrate away, depending on stimuli that we do not as yet understand. Individuals of the same body of fish may emigrate toward Nova Scotia, to the shores of Maine, or to South Channel and Nantucket Shoals, favoring no particular locality.

Georges Bank and Browns Bank.—A channel over 100 fathoms deep separates these two banks, and there has been some speculation as to whether this acted as a barrier to cod migrating from Nova Scotia and Browns to Georges Bank. Our tagged fish and those tagged by the Canadian Government have proven, however, that some cod from Nova Scotia and Browns Bank do migrate to Georges and even to as far as Rhode Island. Most of the Browns Bank tagged cod migrated to Nova Scotia, particularly along the east coast, while one fish went east as far as Western Bank in longitude 61°.

So few pollock have been tagged and so few recaptured that nothing definite can be said concerning migrations. Pollock are not as stable in their habits as cod, for while we found them plentiful on Nantucket Shoals in 1923 they have not reappeared there since that time except as stragglers. They migrated away from the southern Massachusetts region over the winter of 1923–24. Some pollock remain on the same ground from one year to the next, while some make long migrations. Fish of about the same size school together, and even large fish 30, 35, or 40 inches long remain together in small schools. A few pollock migrate westward as far as New Jersey over the winter, but they are not of commercial importance west of Rhode Island.

Haddock.—With less than 200 recaptures from over 10,000 tagged haddock, nothing definite can be as yet said about the migrations of this fish. It is obvious that haddock must migrate to the South Channel region, else the very intensive fishery that exists there at all times could not continue.

According to our results so far some haddock on Nantucket Shoals remain there from one year to the next, but most of them migrate to the Chatham–South Channel region sometime between summers. A few haddock migrate westward in the fall, along with the cod, to as far as northern New Jersey, while an occasional fish goes as far south as Cape May; but there is no commercial fishing for haddock farther west than the eastern part of the Long Island coast.

A large part of the marked haddock living in the immediate shore waters of the Gulf of Maine did not migrate within a year after tagging, but of those that did migrate more went southward to the South Channel region than went eastward to or toward Nova Scotia. It would seem that as haddock are caught up in South Channel new fish are drawn from all parts of the Gulf of Maine to take their place.

A complete report embodying the results of five years' investigations of the cod is virtually completed and will be presented for publication in the near future. These investigations have been conducted with the advice and under the general supervision of Dr. H. B. Bigelow of the Museum of Comparative Zoology, Harvard University.

SMELTS OF NEW ENGLAND

During the progress of the study by Dr. W. C. Kendall of the various problems pertaining to the smelts of New England, particularly as concerned relationships, it was found necessary to take into

consideration the species of other countries, especially those of Europe and the Arctic regions. But few data of the kind to afford the desired information were available, and sufficient material for study at this time was lacking. However, comparisons of data pertaining to the smelt of eastern North America were made with scant similar data relating to the smelts of Europe, Siberia, and Alaska. Of the characters compared none was found that did not intergrade more or less, so that differences, if any, were indicated by averages only. Therefore, according to taxonomic usage each of the several divergent forms would be regarded as subspecies of the earliest described form, unless other characters should be found to distinguish them. Some European ichthyologists have disposed of them in that way. Apparently, however, the intergradations are not of such nature that they can be dismissed thus early, for the taxonomic definition of what constitutes a subspecies does not conform to what appears to be the rather complicated natural situation, for the complex of variations and intergradations is such that the same fish may be regarded as a subspecies of any one of two or more other forms, depending upon which characters are taken as the basis for the conclusion. The situation becomes still more involved because it does not conform to the taxonomic definition of species.

Therefore, since names are restricted to binomials and trinomials for species and their minor divisions, and since classification is for convenience, the simplest and most natural course to follow would be to regard the most widely divergent forms as entitled to binomial rank, regardless of intergradation. Thus, the species would be distinguished by an ensemble of characters varying in different directions or differences of degree of variation of characters. Even then there would be minor divisions of such ensemble species to consider as subspecies. The practical value of classification is not alone that of labeling specimens in a museum, or in the publication of faunal catalogues, but also in its economic and fish-cultural application. For example, there may be cited the one-time question concerning the relationship of the Atlantic and Pacific sardines (pilchards) or the still unsatisfactorily answered question concerning the western trouts.

As pertains to the smelts of this country, the economic problems can not be answered by any system of classification, but that of the relationship of the fresh-water smelts is of fish-cultural importance particularly as pertains to the popularly designated large and small smelts. Notwithstanding the overlapping of sizes and other puzzling differences, which we are not warranted in calling intergradation, there remain the distinctive facts separating the large and small forms: (1) Difference in spawning time. (2) Difference in character of food, so far as observed. (3) Differences in ages of breeding fish.

That this last fact is of some considerable significance is indicated by the establishment of large smelt from Green Lake in Crystal Lake, Michigan, in remarkable numbers, without any indication of the small category. Furthermore, both Creaser and Metzelaar found a few 2-year-old smelts much larger than the 4-year small class of Green Lake, but the majority of breeding fish were 3 years old. Thus, it appears to be established that the two categories of " small "

and "large" smelt are at least physiological divergents, whether or not they can be regarded as distinct taxonomic species with respect to differences of structure.

In the spring of 1928 the observations upon the spawning runs of salt-water smelt, which had been carried on in two brooks in Freeport, Me., from 1924, were unavoidably interrupted, so that but few were made. This year the run was somewhat larger than in 1927, and there was a greater proportion of 2-year-old fish.

During the year the work on a report embodying the scientific results of this investigation has progressed. Recommendations for regulation of the smelt fishery and the abatement of abuses of wasteful and destructive fishing, which have led to serious reduction of the supply of smelt in Maine, were again offered to the State legislature.

MACKEREL INVESTIGATION

The outstanding peculiarity of the mackerel fishery is its wide fluctuations in productivity. At the beginning of the investigation in 1925 there was considerable doubt as to whether these reflected the abundance of mackerel or were due to other causes, such as local movements of this fish; but the past several years have proven that the catch does, in fact, depend largely on the abundance of mackerel. Moreover, it has been found that the changes in abundance are due to inequalities in the contribution of each spawning season to the stock in the sea, hence age domination has been evident in highly accentuated form during the three years that this fishery has been under observation, and it has been shown that the unusual abundance of mackerel during the period 1925–1928 was caused by the exceptionally successful 1923 brood, which accounted for about 90 per cent of the catch throughout most of this period.

Having discovered the nature of this phenomenon, our attention has been more recently directed toward refining our methods of observation of the fishery in order that a fairly accurate estimate of future abundance might be made as a service to the fishery. To do this we have used both statistical and biological approaches to the problem in a form of cooperation between the divisions of fishery industries and of scientific inquiry, which has proven effective. During 1928 the activities continued under the direction of Oscar E. Sette, assisted by Edward W. Bailey. Most of the observations on the mackerel at landing ports were made by Frank E. Firth, while special data on trap catches were collected by Robert A. Goffin. At the mackerel-landing ports at Cape May, N. J., New York City, and Boston and Gloucester, Mass., vessel captains were interviewed with respect to date, locality, quantity, and other items relating to their catches. The fish in a sample of 20 or more from most of the fares were measured, and scales were taken from lots of five fish from many of these samples. In this manner the observers secured data on 1,240 trips out of 2,517 landed by the fleet during the season. Samples totaling 32,294 mackerel were measured from 858 of these trips, and several thousand scale samples were collected.

These and similar data collected in previous years have demonstrated domination by the 1923 year class during the period 1925 to 1927 and made possible a forecast in advance of the 1928 season,

the prediction being that a decline of at least 12½ per cent in abundance would occur between 1927 and 1928. In terms of catch, this meant a reduction from 42,000,000 pounds in 1927 to something less than 37,000,000 in 1928, provided the fishing effort remained the same in both years. This prediction proved to be approximately correct, for the catch in 1928 totaled slightly over 31,000,000.

The success of this prediction must be credited to the simplicity of conditions rather than to a profound knowledge of factors affecting the fishery. We had only to consider the 1923 brood in making the 1928 prediction, and since this had been subject to a decline in the previous year it was apparent that a further decline might be expected. The first tow-netting for mackerel eggs and larvæ was done in 1926, and in the past three years enough exploratory work has been done to define roughly the important spawning areas and to give a general idea of the abundance of mackerel eggs and larvæ to be expected in those areas.

Two cruises were made by the *Albatross II* in 1928. The first one, in May, covered the continental shelf from Cape Cod to Delaware Bay between the 10 and 100 fathom contours. Thirty-nine stations were occupied, and townet hauls were made at 37 of these. Analysis of the collections indicated mackerel eggs and larvæ in abundance nearly equal to that found on the comparable cruise of 1927. A second cruise in July included stations in the waters north, east, and south of Cape Cod within 50 miles of the shore line. The spawning season being past at this time, very few mackerel eggs were found in the tow-net hauls, but larvæ were taken in abundance in Massachusetts Bay and at stations just south of No Man's Land. While we can not yet express the abundance of mackerel eggs and larvæ in terms of expected yield of commercial mackerel two years hence, a longer series of data of the kind that indicates the abundance of commercial mackerel realized from a certain concentration of mackerel fry in previous years may eventually lead to a solution of the forecasting problem.

The results of the studies on abundance of the mackerel in its early stages may be even more significant in explaining the causes of age dominance. An estimate of the abundance of eggs and larvæ through a sufficient number of years should disclose whether brood failures are due to a dearth of mackerel eggs, unusual mortality of larvæ, or failure of later stages to survive. Many marine fishes are subject to fluctuations in abundance resulting from unequal year broods, and to find the cause in the case of any particular species would constitute a distinct advance in fisheries science. The mackerel, with its sharply contrasted "good" and "poor" broods, appears to be a particularly favorable subject for the study of the factors responsible for nonsurvival of certain broods, hence this phase is to receive considerable attention in future mackerel investigations.

SHORE FISHES OF THE MIDDLE ATLANTIC STATES

Studies of the fisheries of the Middle Atlantic States, begun in July, 1927, were continued and extended in 1928 by R. A. Nesbit, assisted by H. A. Hanson, H. M. Bearse, W. C. Neville, V. E. Heffelfinger, A. C. Redmond, Gerritt Bevelander, and Edward Bevelander.

Valuable assistance was rendered by C. M. Breder, jr., of the staff of the New York Aquarium. The Bingham Oceanographic Collection cooperated with the bureau in this investigation, A. E. Parr being engaged in biometric studies of races of squeteague.

The ultimate object of the investigation is to determine the causes of changes in the yield of these fisheries from year to year, with particular reference to separation of the results of natural changes in abundance from the results of possible overfishing. In order to accomplish this aim it has been necessary, as in 1927, to carry on two lines of investigation, the first to determine what changes have taken place in the past and are taking place at the present time, and the second to determine the causes of the changes observed.

In carrying on the work, statistical records and biological data have been collected for each of the more important food fishes taken in the region. Analysis and interpretation of the data collected has, for the present, been confined largely to squeteague, the most important species. Discussion of observations and results will therefore refer to this species.

The first line of investigation has been carried out by securing records of the catch of former years from log books and bookkeeping records, lent by fishermen from a number of localities, and by instituting a system of voluntary statistical returns for current catches. Leading pound-net operators have cooperated readily, so that about 80 per cent of the pound-net catch in New York and New Jersey was reported daily during the 1928 fishing season. Daily reports are particularly valuable, since it is important to note the magnitude of the several distinct runs that occur during the season, particularly as the runs show marked differences in age composition.

Comparison and analysis of the records of former years indicate that even though incomplete they provide a dependable index of change in abundance over the period covered. It was hoped that the range and character of the fluctuations, as shown by these records, would present unequivocal evidence of a definite trend in the yield. Unfortunately, most of the records cover only the period from 1920 to 1928. The general downward trend in New York and northern New Jersey contrasts with an upward trend in southern New Jersey for these years. A single record from eastern Long Island, N. Y., indicates that prior to 1900 the yield per trap was only moderate, but that between 1900 and 1908 a tremendous increase took place, followed by a decline to lower levels. Statistical canvasses indicate that the phenomenon was a general one for the whole of New York and southern New England. Since some species are known to fluctuate violently from natural causes, with lapses of as much as 40 to 60 years between periods of extreme abundance, it is possible that the present low level of abundance of squeteague in New York and southern New England waters is due to natural causes from which a natural recovery may be expected. Hence, it is not possible at this time to state definitely that the decline is due to overfishing.

Biological studies intended to discover the causes of fluctuations in the yield were designed to detect variations in the annual increment to the population. This seemed the most promising line of

investigation, as dominant year classes have proved to be the most common cause of natural fluctuations in other fisheries.

Field bases were established at Montauk and West Sayville, N. Y., and Belford, Long Branch, and Wildwood, N. J. In addition, collections of field data were made by the laboratory staffs at Woods Hole, Mass., and Beaufort, N. C. Random samples of fish of the more important species were measured daily during the season, and scale samples were taken from those species that appear to have legible scales. In all, about 150,000 fish were measured and about 8,000 scale samples were collected.

Tabulation and comparison of the squeteague measurements from the several field bases have shown marked differences in the length-frequency composition of the runs in each locality sampled, as well as sharply defined seasonal differences in each locality. In general, the smaller sizes were taken in the more southerly part of the region and the larger sizes off northern New Jersey and New York. Comparison of the 1928 frequency distributions with the fragmentary records obtained during the 1927 survey shows that for corresponding dates and localities the distributions are nearly identical. Should future observations confirm the suggestion that there is a characteristic size group for each phase of the season in each locality, study of dominant year classes, if such exist, will be greatly complicated.

Scale studies are now being carried on to determine the percentage age composition of the size groups observed and to determine and compare the rate of growth of the fish composing them. At the same time a critical study of the validity of the scale theory, as applied to squeteague, is being made. Assuming, subject to verification, that the scale interpretations are correct, considerable differences in the rate of growth of fish making up the several distinct size groups have been noted. These differences exist, not only between different localities, but scale records from fish from the same localities at different times in the season show considerable differences.

In July and August a survey of Chesapeake Bay was made, preliminary to extension of the program of regular field-data collection to this region. Field bases for 1929 were selected. During the course of the survey several thousand fish were measured for comparison with measurements to be made in 1929.

Extension of the program for 1929 includes an investigation of spawning areas and studies of the growth of young fish in cooperation with the Bingham Oceanographic Collection.

SHORE FISHES OF NORTH CAROLINA

The program of the investigation of the eggs and larvæ of commercial fishes, announced in the last annual report as a preliminary step in the study of the biology of survival, has been continued by Dr. S. F. Hildebrand, assisted by J. S. Gutsell and Louella E. Cable at the Beaufort (N. C.) fisheries biological laboratory throughout the year. The immediate object of this work is to secure a complete developmental series of the important fishes of the region in order to facilitate their ready identification, to study their distribution and abundance throughout the year, and to collect such other biological information concerning their development as may be

necessary for an understanding of the problems of their conservation.

One offshore trip, when weather conditions permitted, was made each week, and other collections were made in inshore waters. In offshore work meter townets were used almost exclusively, and an equal number of collections was made at the surface and bottom. Inshore work with meter townets was supplemented by collections made with an otter trawl, the "cod end" of which was covered with bobbinet. The last-mentioned apparatus brought in some greatly desired "intermediate sizes"; that is, specimens larger than those usually taken in meter townets and smaller than generally appear in collecting seines and trawls.

In addition to the collections made locally, many specimens of young fish collected in southern waters, principally by the late William W. Welsh, on cruises made by the *Bache* and the *Albatross* were transferred to this station for study.

The material at hand has not all been studied. To acquire a complete series, showing the development from the egg to the adult stage, generally is difficult. However, at least some of the stages in the development of 20 species, not studied previously with respect to the development of the young, have been secured and figured.

In 1928 scale studies relative to age determination and the rate of growth of the pigfish (*Orthopristis chrysopterus*) were undertaken by Louella E. Cable at Beaufort, N. C. Material for this work has been decidedly limited. However, it is hoped that more extensive observations may be possible soon. The food of the pigfish has been especially studied throughout the year, the alimentary tracts of more than 100 fish having been examined.

TEXAS FISHES

With the printing of Document 1046, Natural History and Conservation of the Redfish and Other Commercial Sciænids of the Texas Coast, by John C. Pearson, at the end of 1928, the bureau's intensive field work on the coastal fisheries of Texas was completed, and only laboratory studies on collections previously made were continued. In connection with the systematic study of the fish fauna of the Texas coast, undertaken last year by Isaac Ginsburg and arising from the identifications of species collected by field parties, several problems in systematic ichthyology have arisen. The problem of the specific identity of the sand trout of the Gulf coast was solved by a study of large series of specimens. In this study it was found desirable to make also an extensive study of the gray squeteague of the Atlantic coast in order to compare it with the Gulf coast fish. It has been definitely determined that the so-called sand trout consists of two entirely distinct species, which, although superficially alike in appearance, may be easily separated by well-marked structural differences. The two species vary in size and also in their market value. The smaller species is *Cynoscion nothus*, which also occurs on the Atlantic coast; the larger and more important market species is very close to *Cynoscion regalis* from the Atlantic coast but differs from it sufficiently to be regarded as a distinct species. This species is

apparently as yet unnamed scientifically. The name *thalassinus*, which was provisionally used for it, is evidently untenable. A report on this investigation is in press.

GREAT LAKES FISHERY INVESTIGATIONS

The fishery investigation on the Great Lakes during 1928 was largely a continuation of the program inaugurated in 1927 on Lake Erie. Some fisheries work was carried on also on Lakes Huron and Michigan. In 1928 extensive limnological studies were begun on Lake Erie.

LAKE ERIE

1. *Fishery investigations.*—The work on Lake Erie may be considered conveniently in four categories: (1) General survey, (2) destructiveness of commercial gear, (3) selective effect of gear on the general population, and (4) biology of commercial species. In the general survey information was obtained on the history of the fisheries, the kind and amount of gear now employed, the size of meshes and twine and the depth and length of nets now in use, the location of fishing and spawning grounds, the spawning seasons, the abundance of fish (statistics), the migration and other habits of fish, probable causes of depletion and desirable legislation on size limits, closed areas, closed seasons, gear, etc.

In the study on the destructiveness of gear, data were obtained on the percentage of illegal or small fish taken and destroyed by the commercial gill net, bull net, trap net, pound net, and seine during various seasons of the year and in various areas of the lake. In order to determine whether it is possible to reduce the destructiveness to the gear now fished, two series of experimental nets were operated, one comprising shallow gill nets of various sized meshes and the other trap nets with various sized meshes in the back or head of the lifting crib. These experimental nets were fished throughout an entire fishing season to test all conditions in the fisheries, which vary greatly with the seasons. By these means many data were obtained on the selectivity exercised by the different-sized meshes on the various populations of fish.

Detailed studies have been begun on the biology of the nine most important commercial species of fish. Such studies are based on data on age, growth, sexual maturity, longevity, mortality, spawning, migration, relative abundance of males and females, fluctuations in abundance of size groups and year classes, in sex composition of schools, and in the relative abundance of the different species in the commercial lifts and on races.

The work was carried on in the field by members of the bureau's Great Lakes staff, who were assigned to various ports on the lake and under the direction of Dr. John Van Oosten. The following investigators were engaged on Lake Erie: Dr. Stillman Wright, Ben Hill, C. Willard Greene, and Hilary J. Deason.

Though the field work was carried on jointly by the various investigators, the laboratory work was apportioned among them, and each investigator was assigned a certain definite problem. Doctor Wright is carrying on a study of the correlation of growth

and environment in several species of fish from a series of Wisconsin lakes. Mr. Greene is engaged in a study of the natural history and of the life history of the sheepshead as determined from the scales. With the depletion of the more valuable coregonids, the sheepshead is growing more important in the fishing industry. Mr. Deason is investigating the systematic status and the life history of the yellow pike, blue pike, and sauger, while Mr. Hill is analyzing biological data (scales, lengths, etc.) on the white or striped bass. Doctor Van Oosten is compiling the more general data collected in the field, such as those on the destructiveness and selectivity of gear and on legislation, and in addition is continuing his life-history studies on the cisco and whitefish of Lake Erie.

2. *Limnological investigations.*—In addition to the above fishery survey, the bureau cooperated with the game and fisheries department of Ontario, the New York department of conservation, the Buffalo Society of Natural Sciences, and the City of Buffalo in the most extensive limnological investigation ever undertaken on the Great Lakes. This survey was conducted on Lake Erie under the direction of Dr. Charles J. Fish of the Buffalo Society of Natural Sciences. The bureau's vessel *Shearwater* was equipped for the work. This work is discussed further elsewhere in this report.

LAKE HURON

It has been determined from biological studies that the lake herring of Saginaw Bay is subjected to intense fishing. This species is now wholly unprotected, and it is purposed to establish a size limit of 10 inches for its protection. As the herring is a very sensitive fish, it can not withstand the handling due to sorting, hence the only feasible method of protection seemed to be a regulation of the mesh of herring pound nets. To ascertain the mesh required to permit the escape of herring under 10 inches in length a series of pound nets of different sized meshes were fished on herring grounds in Saginaw Bay during November. This work, which will be resumed in the spring, was conducted by Doctor Van Oosten in cooperation with the department of conservation of the State of Michigan, which furnished the experimental nets and three assistants.

LAKE MICHIGAN

In cooperation with the department of conservation of the State of Michigan Doctor Van Oosten also began a preliminary study of the destructiveness to trout and the selectivity of chub nets of different sized meshes on Lake Michigan. The experiment was designed primarily to ascertain which of three meshes, 2½, 2⅝, or 2¾-inch, is most destructive to immature lake trout, a question of paramount importance on Lake Michigan at the present time. Due to severe storms, too few data to be conclusive were collected, but it is hoped that the experiment can be continued next year.

PACIFIC COAST AND ALASKA FISHERY INVESTIGATIONS

The investigation of the Pacific salmon has been seriously affected by the loss of Dr. Charles H. Gilbert, who died early in the year. Doctor Gilbert was responsible for the initiation of this work and

took an active part up to the time of his death. In large measure the present program was planned by him, and much of the credit for future results will be due to his ability to plan such a program of research along broad scientific and practical lines.

ALASKA SALMON

Tagging experiments.—Only one small tagging experiment was made during the year, when 462 tags were attached to red salmon taken in a trap at Nicholaski Spit on the western shore of Pavlof Bay, Alaska Peninsula. The tagging was done on July 11, 12, and 17. There was some reason to suppose that fish caught in this trap were, in part at least, derived from the Bristol Bay runs, and the experiment was undertaken to test this theory. The new regulations for 1928, however, prohibited fishing in this locality prior to July 5, and it is known from previous experiments that most of the Bristol Bay fish have passed out of the region south of the peninsula by the middle of July. Only one tagged fish was recaptured in Bristol Bay, but others were taken at Ikatan and Morzhovoi Bay, possibly en route to Bristol Bay. It seems probable that the trap in question, if fished during the height of the run, would catch appreciable numbers of Bristol Bay red salmon; but under the present regulation it will not, apparently, intercept many of these fish. Four tagged fish were taken at Chignik and others in localities close to the point of tagging. This work was done by L. C. Wingard, assistant agent in the Alaska fisheries service, under the direction of Dr. W. H. Rich.

A report covering the experiments of 1927 and 1928 is in press.

Statistics of the Alaska salmon fisheries.—The compilation and analysis of the statistics of the Alaska salmon fisheries has been continued by Doctor Rich and E. M. Ball, assistant, Alaska service. A first part, dealing with the fisheries of Bristol Bay and the Alaska Peninsula, has been published, and material progress has been made in the preparation of a second part, which is to take up the data for as much of central Alaska as can be covered in the time available. The results of this work have proved to be of great practical value by showing more clearly than has been shown before the condition of the fisheries in each locality. The data clearly show serious depletion in certain restricted localities and to a lesser degree depletion, as indicated by reduced catches, appeared to be rather widespread. By means of correlation studies it has been possible to show a close association between the fisheries in the Ikatan–Shumagin Island district and Bristol Bay, thus corroborating the results obtained by tagging experiments in 1922 and 1923. Other studies based on the correlation between catches of red salmon in Bristol Bay at 4, 5, and 6 year intervals indicate that the Nushagak fish are predominantly 4 years old, while those taken in the Kvichak–Naknek–Egegik fishery are predominantly 5 years old.

Karluk River investigations.—Special and intensive study of the Karluk River red-salmon runs has been carried on for several years. The primary purpose is to determine the relationship between spawning population and the resultant production of adult fish and, further, to determine the factors that may modify the production from a given spawning population in different years. To this end the

spawning population (the escapement) has been enumerated each year since 1921 by means of a counting weir located near the mouth of the river; the commercial catch has also been enumerated, a procedure made possible by the restricted character of the fishery; and the catch each year is carefully sampled, and age determinations are made so that the correct proportions of each run may be referred to the brood years from which they are derived. As the Karluk red-salmon runs contain appreciable numbers of 4, 5, and 6 year fish it follows that we can not know the production of any particular escapement until 6 years have passed, and we have now, therefore, complete returns from only two of the known escapements—those for 1921 and 1922; but from now on we will add each year one figure to our series of data on the production of known escapements. It will undoubtedly require many years before we will have a sufficient number of data to warrant final conclusions, and in the meantime these investigations must be carried on as a matter of routine. Only in this way can we expect to understand the escapement requirements necessary to maintain the runs at the point of maximum productivity, and the importance of the problem is such as to fully warrant the establishment of such a long-time study as this must prove to be.

As a result of these studies it has been shown that the escapement of 1921, which was 1,325,000, produced about 4,250,000, approximately 3 to 1; and the escapement of 1922, which was close to 400,000, produced 2,000,000, approximately 5 to 1. Although the 6-year fish from the 1923 spawning are still to come, it appears probable that the product of that escapement, 635,000, will be intermediate between those of the other two. Even these few data indicate strongly that the smaller escapements are relatively more productive than the larger ones, a fact that may well explain the increased resistance to further reduction of depleted fish populations that has been noted by various students of fishery problems. They also emphasize the importance of determining the point of maximum productivity, because although the smaller spawning populations are relatively more productive the larger ones are actually more productive up to a certain point.

Observations on the spawning grounds of Karluk Lake were continued during the past year. Two trips to the lake, one in July and the other in September, were made by Seymour P. Smith and Alan C. Taft. The conditions for spawning appeared to be very favorable; the water in the spawning streams being higher than has been observed for several years. Additional collections of young salmon were made, and limnological data, including quantitative plankton catches, temperatures, and water residues, were secured. The limnological data are being studied by Dr. Chancey Juday of the University of Wisconsin.

The marking experiments at Karluk, in which the seaward migrants are marked by clipping certain fins, were continued. These have been explained in previous reports and are designed to secure information on which to base an estimate of the number of seaward migrants derived from each spawning escapement. Fifty thousand migrants were again marked in 1928. Very satisfactory returns were secured in 1928 from the marking experiment of 1926.

The divisional report for 1927 contained an account of the returns secured in that year from the experiment of 1926, and on the basis of the few 1927 returns it was estimated that the total seaward migration consisted in approximately 13,000,000 fish. On the basis of the much more complete and reliable returns secured in 1928 it is now estimated that the seaward migration of 1926 contained about 8,125,000. Over 1,400 marked fish were actually recovered. These data indicate that the survival of the seaward migrants, once they reach the ocean, is much higher than has ever been reported. The greatest returns secured from the various marking experiments on the Columbia River indicated a survival of about 1 in 20, but the survival of the Karluk migrants of 1926 is apparently greater than 1 in 4 and possibly as high as 1 in 3. A few marked grilse from the experiment of 1927 were secured, but the number was so small that any estimate of the total number of seaward migrants in that year is unwarranted.

Chignik River investigations.—A study of the red-salmon runs to the Chignik River has been undertaken along somewhat similar lines as that at Karluk. Here, as at Karluk, a weir has been maintained since 1922, and it is possible, also, to get a reliable estimate of the number of fish taken by the commercial fishery. This study is being made by Harlan B. Holmes, assistant aquatic biologist, who devoted the greater part of the past season to a study of the seaward migration of the fingerlings. The seaward migration at Chignik is peculiar in that it is distributed over a period of 2 or 3 months, whereas in other rivers with which we are familiar the migration is of much shorter duration. The problems relating to the red salmon of this river are proving to be of unusual complexity. The scales present irregularities that can not be interpreted with certainty until a detailed study has been made of the growth of the fingerlings and the development of their scales. The task of determining the number of fish of each age in the spawning migration is complicated by pronounced and irregular fluctuations in the age composition of the run at different times of the season. The study is further complicated by fluctuations in the time at which the bulk of the run is passing the commercial fishery. The run is in progress for approximately 20 weeks, but half or more of the fish may pass during a period of two weeks. This peak in the run may come as early as June or as late as August. The run is not always concentrated into a pronounced peak; in some cases it has remained quite constant for a period of 10 or more weeks. As a result of these irregularities in age composition and time of the run, scale samples upon which an age analysis of the run is to be based must be much more extensive than those required in cases in which these conditions are less variable. The problems of sampling and scale interpretation are to be given special attention next season.

COLUMBIA RIVER SALMON

Salmon-marking experiments, which are being conducted on the Columbia River in cooperation with the Oregon Fish Commission, have been continued under the supervision of H. B. Holmes. One new experiment was started during the season. This was designed

to determine at what age fingerlings of the fall run of chinook salmon should be liberated in order to give the greatest number of resulting mature fish. It may be argued that since the young of the fall run of chinooks normally migrate to the ocean soon after they hatch, it would be best to permit the hatchery-reared fingerlings to migrate at that time. On the other hand, it is possible that older and larger fingerlings would have greater chance of survival and that it would be better to retain the young fish at the hatchery for a part or all of the first season. The experiment, which is intended to give some information on this subject, consists of liberating groups of fingerlings at different times of the season, each group being distinctively marked by the removal of certain fins from each fish. A similar experiment has already been applied to fingerlings of the spring run of chinook salmon. These marked fish were in their fourth year in 1928 and a few were recovered. The greater part of the fish from this experiment are expected to return in 1929 as 5-year olds.

Marked salmon recovered during the season of 1928 include 21 specimens from an experiment with the landlocked salmon *Oncorhynchus nerka kennerlyi*, commonly known as the little redfish or "yank." These fish were marked for the purpose of determining if it is possible to induce this landlocked variety to become sea run. Practical interest in this experiment attaches to the fact that these fish in their native habitat attain a size of only about one-half pound and are of very little economic value. If they could be induced to become sea run, and if, like the more typical salmon, they would respond to the ocean environment by making a vigorous growth, they could be made to contribute to a valuable resource.

The marked landlocked salmon were in their fourth year in 1928. The 21 specimens that were reported were caught in the Columbia River by commercial fishermen. As far as could be determined by a superficial examination, these specimens seemed to be identical with their nearest relative, the blueback salmon. This resemblance was to be expected, because these two forms have not been shown to differ except in size and life history. With these differences eliminated we should expect them to appear identical. This similarity has led to a possible error in the experiment. Fingerlings of both the landlocked and sea-run forms were reared at the hatchery at which this experiment was conducted; and although extreme precautions were taken to prevent intermingling of the two forms, it is possible that these precautions were not successful and that a few of the sea-run form were marked. It is possible, then, that the 21 marked fish that were recovered were from sea-run parents.

Were it not for this possible error in the experiment, these 21 recoveries could be accepted as reliable evidence that the little redfish can be induced to become sea run, but as it is very little confidence can be placed in the results. This experiment is now being repeated under conditions that will eliminate the chance of confusion in the identity of the fish.

EMBRYOLOGY OF THE CHINOOK SALMON

This study has been continued during the past year by Dr. G. C. Price at Stanford University. In the last divisional report mention was made of the fact that this study had shown that the temperature-

unit system for recording development, which is in common use at the hatcheries, is unreliable because the rate of development is not uniformly accelerated by a rise in temperature. During the past year still another complication has been discovered, in that the accelerating effect of a given change in temperature does not act uniformly at all stages in development. It is quite different at early stages than at late stages. It will be impossible to determine the exact relationship between temperature and rate of development without additional data. A report covering the general features of development and presenting the available data on the influence of temperature is in course of preparation.

ALASKA HERRING

The herring investigations commenced by George A. Rounsefell in 1925 have been continued. Owing to the failure of the runs in central Alaska the sampling of catches in this district, from which a series of data was desired over several years, was not as extensive as had been planned. Samples were obtained from several new localities—Puget Bay, Snug Harbor, and Port Fidalgo in Prince William Sound and Zachar Bay and Shearwater Bay on Kodiak Island. Analysis of the vertebral count of these samples indicates that on Kodiak Island the Shearwater Bay herring are distinct from those of Shuyak Strait, thus indicating that the failure of the fishery in the latter locality was not due to any shifting of the schools.

In 1928 the central Alaska herring fishery underwent the greatest failure since the beginning of the industry. In Prince William Sound the usual early summer run in Elrington Passage failed to materialize. Quantities of very small herring, fit only for oil and meal, were taken in Snug Harbor for a few days during the last of June, and small intermittent catches were made during the summer at Macleod Harbor, on Montague Island; but the autumn run of large herring, on which Prince William Sound has been largely dependent for its supply of raw material for pickling, was an utter failure. There has been a decrease from 90,000 barrels of pickled herring in 1922 to about 3,000 barrels in 1928. In Cook Inlet the fishing was also a failure. The August run of large herring in Kachemak Bay, which has been the mainstay of the fishery since the commencement of purse seining in Cook Inlet in 1924, failed to appear. Late in the fall a few catches were made of herring that were unusually small for this area. The purse seiners gathered at Shuyak Strait in July, but caught nothing, although 50,000 barrels of herring were pickled in this area in 1925. Late in the fall a fair run of large herring occurred at Shearwater Bay, as in 1927. This was the only run approaching former size in all of central Alaska.

The almost total failure of the fishery in central Alaska was only partially compensated by the discovery of quantities of herring about 700 miles southwest of Seward at Unalaska in the Aleutian Islands. For many years herring have been known to occur at Unalaska, and very small quantities have been salted from time to time; 1927 was the first year in which they were noticed to be unusually abundant. Owing to the almost complete failure of the central Alaska fishery in 1928, the herring packers, scouting along

hundreds of miles of coast, discovered this abundance at Unalaska. Boats and gear were rushed to the scene and 25,000 barrels were pickled, the pack being limited chiefly by the lack of sufficient facilities for handling. An explanation of this unwonted abundance at Unalaska is given by an age analysis of a portion of our samples, indicating that about 70 per cent of the catch were 6 years of age, all of the other age classes combined making only about 30 per cent. This would imply that the appearance of such enormous quantities of herring at Unalaska was due to the survival in 1922 of unusually large numbers of fry. That such enormous differences in the numerical importance of age classes are not unusual has been shown by our analysis over a 4-year period of the ages and lengths of herring from Prince William Sound.

The catches at Unalaska were virtually all made inside of Dutch Harbor, a tiny bay 1½ miles in length by one-half mile in width and only 18 fathoms in the deepest spot. This exemplifies a peculiar situation found in the Alaska herring fisheries. Over hundreds of miles of coast herring will be abundant only in certain bays. All of the gear is concentrated in a limited area, and enormous catches result, with a consequent reduction in the population.

Preliminary analysis of growth rates shows enormous differences between the localities considered. The Shumagin Island and Unalaska herring are about equally fast growing. The Unalaska herring at 6 years of age will weigh 19 per cent more than those of Halibut Cove, Cook Inlet; 62 per cent more than those of Elrington Passage, near Latouche; and 280 per cent more than those of Stephens Passage, near Juneau.

Analysis of preserved material from southeastern Alaska shows the existence of separate stocks of herring at Craig on the west coast of Prince of Wales Island, in Stephens Passage near Juneau, and in Chatham Strait. During 1929 an additional investigator will be assigned to the herring investigation, allowing the field work to be extended so as to include this most important section of the fishery.

CONSERVING FISH BY MEANS OF FISH SCREENS AND FISH LADDERS

For years the great salmon industry of the Northwest has suffered from the loss that occurs when the young fish on their migration to the sea enter irrigation and power diversion ditches and are destroyed. Among migratory game fish a similar loss occurs. Further loss of fish life is occasioned when the mature fish, journeying upstream to spawn, encounter obstacles to their progress in the form of dams and the tailraces of power houses, over or around which adequate fishways have not been provided.

With the situation arising from these destructive influences rapidly becoming critical, a meeting of interested State and Federal agencies, the commercial interests, and sportsmen was held at Yakima, Wash., in January, 1928, and an organization known as the Western Food and Game Fish Protective Association was formed. Recognizing that some of the irrigation projects of the United States Government were occasioning great loss of fish in this manner, representatives of this association went to Washington, D. C., and were successful in securing from the Seventieth Congress the passage of "An act to

provide for the conservation of fish, and for other purposes," appropriating $25,000 to investigate and determine the best means and methods of preventing the destruction of fish occasioned by ditches, canals, and other works constructed or maintained by the United States.

Shirley Baker, a consulting engineer of San Francisco, was appointed to conduct the investigation for the Bureau of Fisheries, assisted by U. B. Gilroy, civil engineer.

During the 6 months since this investigation began, field examinations were made of practically all types of fish screens and fish ladders then in operation or abandoned because of failure. This examination embraced the States of Washington, Oregon, Idaho, and Montana. Numerous designs and models submitted to Federal and State fish and game bureaus were studied, and such pertinent literature as is available was examined. In the case of the electric fish screen the fundamental scientific studies of Prof. F. O. McMillan, department of electrical engineering, Oregon Agricultural College, and Y. E. Yates, of the Pacific Power & Light Co. at Portland, were especially useful, and upon the Tieton Canal of the Yakima reclamation project full-scale experiments were made with an electric screen operating under actual field conditions.

The preliminary report of the investigators is quoted as follows:

It is emphasized that the successful operation of any of the installations considered in this report will depend upon proper inspection and maintenance. Responsibility for the care of any installation should be definitely fixed.

Fish ladders.—For dams up to heights of 50 feet we believe that fish ladders can be made effective if properly designed. Every fish-ladder installation is an individual problem, but the cardinal points working for the success of any fish ladder are (1) location of the entrance in water which the fish naturally seeks, (2) pools of adequate size and free from sharp or constricted turns, (3) proper regulation of flow through the pools.

At high dams (100 feet or more) we have found no fish ladders or mechanical lifting devices in successful operation.

Fish screens.—The screening of irrigation and power diversions against the entrance of fish should, if possible, be done at the head of the main diversion. Such location of the screening device obviates building and maintaining lengthy by-pass channels back to the main stream and eliminates the necessity of screening any branch ditches.

The small diversions can be positively and economically protected against the entrance of fish by a mechanical screen of the type which constantly revolves in the direction of the current, the motive power being derived from the flow of the ditch.

The success of such a mechanical screen depends upon:

1. Adequate strength of construction and proper housing.

2. Perfect sealing of the revolving screen to the bottom and sides of the ditch.

3. Mesh of the proper fineness to screen out the smallest fish likely to be encountered.

4. The provision of suitable trash racks ahead of the revolving screen to prevent its injury by large drifts or débris.

5. The provision of a suitable by-pass channel through which fish encountering the screen may return to the main stream channel. Experience gained from the Cobb screen, located on the Congdon Ditch near Yakima and successfully operated throughout the 1928 irrigation season, seems to recommend that at least one opening of such a by-pass should be located at the bottom of the ditch, directly beneath the revolving screen.

Any type of mechanical screen which even momentarily allows the screened cross section of the ditch or any portion of same to be open for the passage of accumulated débris under, around, or through the screen is undesirable. * * *. Shutter screens of either the horizontal or vertical type, in which

the diverted water is separated from the main flow by an arrangement of adjustable vanes or shutters calculated to screen out the fish, so far have proved impracticable due to liability to clogging.

Electric screens.—Diversions of any size can be effectively and economically protected against the entrance of fish by means of the electric fish "screen," "stop," or "diverter." The electric field of such an installation can be established and controlled so as to prevent injury to the fish.

The Burkey electric fish diverter, used on the experiments in the Tieton Canal of the Yakima project, has proved itself a practical device.

The proper location for an electric fish screen is outside the head gates, with the electric field set up diagonal to the plane of the gate openings. An ideal location is just behind the shear boom which is commonly placed in front of the gates and at an angle with them for protection against large drift and débris. With the electric screen so placed a fish encountering the electrified field of water in front of the head gates is immediately diverted from further progress toward the gate openings and finds in the entire flow of the stream past the diversion the most natural avenue of escape from unpleasant stimuli.

The electrified zone of water which the fish encounters should be of graduated strength, the electric effect increasing in intensity as the head gates are approached. An electric field set up by a direct current which is interrupted and reversed and which operates at a low frequency (approximately 6 to 10 cycles per second) proved effective at the Tieton experiments.

Electrodes of chain are regarded favorably because of their flexibility, which aids natural cleaning in the passage of small drift and débris.

INVESTIGATIONS PERTAINING TO FISH-CULTURAL OPERATIONS

EXPERIMENTAL TROUT CULTURE

Feeding experiments with trout, which have been carried on for several years at the experimental hatchery at Pittsford (formerly known as the Holden station), were continued during the season of 1928 by Russell F. Lord under the direction of Dr. H. S. Davis.

Because of congested conditions at the hatchery it was impossible to begin the experiments as early as usual, but they were continued somewhat later in the fall than in previous years, which to some extent compensated for the delay in getting the experiments under way. It is unfortunate that, owing to local conditions, it is impossible to carry on feeding experiments at this station throughout the year.

During the summer of 1928 the feeding experiments were confined almost entirely to brook trout. The experiments with fingerling trout were conducted in hatchery troughs as in previous years, an entire trough being allotted to each lot of fish. There were 28 experimental lots of brook-trout fingerlings, each lot originally containing 1,500 fish. In addition to the brook trout, six lots of rainbow fingerlings were kept on experimental diets.

The feeding experiments with yearlings were confined to brook trout, which were held in raceways under as nearly identical conditions as possible. The raceways were divided into 16 compartments, the fish in each compartment being kept on a different diet. The compartments were covered with wire screens to prevent losses from birds or other enemies, which had caused some trouble in previous years. Each compartment contained from 200 to 425 fish, depending on the size of the compartment.

Owing to the continually increasing cost of the fresh-meat products commonly used for feeding trout, the experiments during

the past season were planned primarily to determine what substitute for fresh meat can be utilized most advantageously. Since it is well known that trout will not flourish unless some fresh meat is included in the diet, dry meals in various proportions were combined with the former in making up the experimental rations. In practice it was found best to moisten the meals first with warm water to the consistency of a thick mush and then incorporate it with the ground meat. The amount of water absorbed by different meals varies from 1½ to 2 times the weight of the dry meal. The same amount of water was always used with each meal, so that the percentage of dry meal in the diet would remain constant.

In the experiments with fingerling trout the controls were fed a mixture of equal parts beef heart and beef liver, as previous experience had shown that this ration gives the best results of any of the straight meat diets. Only one of the experimental rations proved to be superior to the beef-liver and beef-heart mixture. This was a mixture of equal parts of beef liver and clam meal, which gave better results than any other diet used, both as regards growth and mortality. Clam meal, which is manufactured from the refuse of clam canneries, has uniformly given the best results of any dry product that has been tested. Unfortunately only a limited supply of this meal is available.

A ration composed of 85 per cent beef heart and 15 per cent dried skim milk gave nearly as good results as the beef-liver beef-heart mixture. Unlike other dry products, it is necessary to add the dry milk directly to the ground meat, otherwise most of it will be lost in solution. This gives a more concentrated ration than any of the others used, which is, no doubt, largely responsible for the favorable results obtained with this mixture. Since the cost of dry skim milk is more than double that of the animal meals, this ration is also relatively expensive.

Haddock meal, shrimp meal, and a superior grade of cod-liver meal also gave satisfactory results when mixed with an equal quantity of beef liver. The experiments indicate that the use of these meals in combination with an equal quantity of fresh meat will form a satisfactory ration for fingerling trout at a considerable saving in cost over a straight meat diet. The results with menhaden meal were less encouraging.

In feeding these dry products the physical nature of the food has an important bearing on the results. The mixture must be of the proper consistency to be taken readily by the fish and also to reduce to a minimum the loss of nutrient constituents by solution. It has been found that in most instances mixtures containing as high as 50 per cent of the moistened meal are of about the right consistency for feeding, but if the amount of meal is materially increased above these proportions the fish do not take the food as readily and a considerable percentage may be lost.

Several experimental lots were fed fresh fish, either alone or in combination with liver. The fish products used were "bone loaf," composed of the waste in filleting haddock and ground herring. When fed straight, both of these products produced only a medium growth, and after several weeks there was a heavy mortality in both lots of fish. Better results were obtained when "bone loaf" was

fed in combination with liver, but the growth was inferior to that obtained with several other rations and the mortality was comparatively high.

The rations fed yearling trout were in most cases quite different from those used in the experiments with fingerlings, since previous experience had shown that trout of this age will do well on many foods that can not be used successfully with the latter. Owing to the high cost of beef liver it was not considered advisable to use it in making up the rations for yearlings, and consequently pig liver and sheep plucks were adopted as the standard meat foods. These products were fed straight and in combination with several animal meals, wheat middlings, and soy-bean meal.

It was found that with both pig liver and sheep plucks much better results were obtained when the meats were combined with animal meals than when they were fed straight. As in the case of fingerlings, clam meal proved to be much superior to any other dry product used. Shrimp bran was next in value to clam meal, while menhaden meal was a poor third. Haddock meal was not used in these experiments. Wheat middlings and soy-bean meal in combination with sheep plucks gave results that were markedly inferior to those obtained with either of the three animal meals previously mentioned.

Pig liver gave somewhat better results than sheep plucks when fed in combination with either clam meal or shrimp bran, although the difference was not great. When fed straight, however, pig liver was much superior to sheep plucks both as regards growth and mortality.

The poor results with sheep plucks were not anticipated and may have been largely due to its physical nature, as there is more waste from light portions of the lungs, which float upon the water and may be carried against the screens or blown ashore. There is also a great deal of indigestible connective tissue. Cooking the plucks was found to be inadvisable, the fish refusing to take them readily. Pig liver appears to be superior to sheep plucks for practical use and costs but little more. When ground, the former is quite wet and very sticky, making it an admirable binding medium for whatever dry foods are added to the ration.

The results indicate that a considerably smaller percentage of fresh meat is permissible in the diet of yearlings than in that of fingerlings. In several instances excellent results were obtained when only one-third of the ration was composed of fresh meat, and it is probable that considerably smaller amounts could be used without affecting the fish adversely. In striking contrast to the fingerlings, yearling trout fed ground mackerel and herring showed a very low mortality, but the growth was much inferior to that obtained with fresh meats combined with animal meals.

It is evident from the results obtained in these experiments that various dry products can be substituted successfully for part of the meat in the diet of trout at a very considerable saving in food costs. It is also apparent that as the fish increase in size considerably larger percentages of the dry products can be used. Since in all our experiments animal meals have given much better results than cereals or beans, it is doubtful if at present prices there is any economy in feeding the latter to trout, either large or small.

Selective breeding of brook trout forms another important phase of the work at the Pittsford hatchery. As there was no stock of fish at this station when it was taken over by the division of inquiry, it was impossible to accomplish anything in this direction during the first two or three years. Some preliminary experiments along this line were undertaken in 1926 and again in 1927, and while the results have been very encouraging, of course nothing of permanent value has yet been obtained. The work has now reached a point, however, where it is possible to undertake experimental breeding on a more extensive scale, and during the spawning season of 1928 thirty-four pairs of brook trout were mated and the offspring of each pair will be reared separately. All fish thus mated are tagged, and a complete record is kept of each individual fish, including weight, body proportions, color, contour, and general condition.

One of the greatest difficulties in trout breeding is to evaluate correctly fish of different parentage. In order to provide a common basis of comparison, as free as possible from error, all fingerlings produced in the breeding experiments will be reared during the first season under virtually identical conditions. These precautions will not be confined to the young of selected pairs, for it is planned to rear representative samples of all lots of fingerling fish under the same conditions, so that direct comparisons can be made in every case.

While the chief purpose of the breeding work is to obtain a superior strain of fish for propagation purposes, it should be pointed out that the data acquired in these experiments will have an important bearing on other phases of trout culture.

PATHOLOGY OF FISHES

Investigations by the pathologist, Dr. H. S. Davis, on the diseases of trout were continued at the experimental hatchery at Pittsford, Vt. Special attention was directed to two bacterial diseases that cause serious losses at many hatcheries.

The gill disease, which was first recognized at this station, is caused by a specific bacterium that develops on the gill filaments, and in the fry is also found on the body and fins. This disease is now known to occur at many hatcheries in the east and middle west, and during the past summer was found in fingerling salmon from the Birdsview (Wash.) hatchery. There is also evidence that it sometimes occurs in wild fish. If allowed to run its course the disease usually causes a heavy mortality, especially among fry and young fingerlings. In some instances there has been virtually a total loss among fish of this age. Fortunately, the disease can be controlled easily by dipping the fish in a 1 to 2,000 solution of copper sulphate if they are treated on the first appearance of the infection.

The investigations during the past summer indicate, however, that it will be very difficult, if not impossible, to eradicate the disease from a hatchery, as it has been found that fingerling trout may become infected from yearlings and older fish that show no evidence of the disease. This indicates that the bacteria may occur in small numbers for an indefinite time on perfectly healthy fish,

which thus act as carriers. This is another illustration of the inadvisability of allowing trout to remain in a spring or pool from which a hatchery obtains its water supply.

Observations made during the past year indicate that "fin rot" is much more prevalent in trout hatcheries than has been realized. Apparently the organism varies greatly in virulence, and in many cases the disease occurs among fingerling trout as a comparatively mild infection, which causes a small daily mortality without assuming the proportions of an epidemic. In such cases a majority of the fish recover from the disease without suffering serious injury, and the fins rapidly regenerate the parts that have been destroyed.

There is evidence that Gyrodactylus may be an important contributing factor in fin rot. This parasite is often found associated with the disease, and it seems probable that fins that have been injured by it are more susceptible to infection than would otherwise be the case.

An epidemic among fingerling trout at the York Pond (N. H.) station was found to be due to infection with *Costia necatrix*, a small protozoan parasite that occurs on the body and gills. This disease has recurred at about the same time each spring among small fingerlings held in raceways supplied with water from York Pond. As the pond contains large numbers of trout of various ages, it appears probable that the parasites are carried from these fish to the raceways.

As in previous years the pathologist was consulted by a number of State and commercial hatcheries regarding the occurrence of parasites and diseases among hatchery fish. Diagnoses were made and treatments recommended, with beneficial results in all cases in which treatment was practicable.

POND CULTURE

The investigations on pond culture under the general direction of Doctor Davis at the Fairport (Iowa) biological station, which were begun in 1926, were carried on in 1928 by Dr. A. H. Wiebe, assisted by the station staff. As in previous years, special attention was devoted to the largemouth black bass, although other species were propagated to some extent.

One of the most important features of the work at Fairport has been the utilization of forage fish in the spawning and rearing ponds. Three years' experience with forage fish has shown conclusively that by their proper use the production of fingerling bass can be greatly increased at little additional expense. No attempt has been made to rear forage fish in ponds devoted solely to this purpose, except in a few instances in order to insure having a supply on hand for stocking the bass ponds. In virtually all of the experimental work the bass ponds were stocked with adult forage fish early in the spring, so that they would have an opportunity to spawn in the ponds and thus provide a supply of small fish for the bass fingerlings.

Further experience with the golden shiner and blackhead minnow supports the opinion expressed last year that the golden shiner is the better of the two species for forage purposes. This is well shown by

the results in the "E" ponds. These are small ponds of approximately the same area and situated side by side. Two of these ponds were stocked with golden shiners and the other two with blackhead minnows. Later 3,900 bass fry were liberated in each pair of ponds. When drawn in the fall there were 1,507 fingerling bass in the shiner ponds with a total weight of 220 ounces. On the other hand, the ponds stocked with blackheads yielded only 307 fingerling bass weighing 116 ounces.

The best production was obtained in pond D–7, which has a surface area of 1 acre. This pond was stocked with 434 adult shiners and 21,750 bass fry. When drained in the fall approximately 10,000 fingerling bass, averaging about 3 inches in length, were taken from the pond, showing a survival of 46 per cent. Only 301 adult shiners were found in the pond, and there were no young shiners present, indicating that the pond was understocked with forage fish. It seems reasonable to assume that if the pond had not been understocked with shiners the yield of bass would have been considerably greater.

Heretofore no attempt has been made to propagate smallmouth black bass at Fairport as conditions in the ponds were not considered suitable for this species. However, in the spring of 1928 one pond, with an area of 0.6 acre, was stocked with 20 adult smallmouth bass and 258 adult golden shiners. When drained in the fall 4,393 fingerling and 19 adult bass were found in the pond. The shiners had disappeared entirely. This is a production of approximately 7,300 fingerling bass per acre, which, under the circumstances, is considered a very satisfactory return.

Quite satisfactory results have also been obtained with both black and white crappie. These fish do well in the same pond with bluegill sunfish, such a combination showing a production of 10,000 to 12,000 fingerling crappie per acre, with an even larger number of bluegills.

Further experience with application of fertilizers to the ponds has fully confirmed the results of the previous year. The fertilized ponds in variably show a much greater production of fish than the unfertilized. The mixture of dry sheep manure and superphosphate adopted in 1927 was used in 1928, also, but in most instances in smaller amounts with equally satisfactory results. Under the conditions that obtain at Fairport it appears that 500 to 1,000 pounds of the mixture per acre of pond area will give virtually as large a production of fish as when greater amounts of fertilizer are used. The fertilizer should not all be applied at one time, but small amounts should be distributed during the spring and early summer in the shallow water around the margin of the pond.

Limnological observations were made on both fertilized and unfertilized ponds and also on small concrete pools devoted solely to plankton production. As a result of these observations extensive data have been obtained on the temperature, transparency, pH, dissolved oxygen, free and half-bound carbon dioxide, dissolved phosphorus, organic phosphorus, nitrogen as free ammonia, as nitrite, as nitrate, and as organic nitrogen, dissolved chlorides, dissolved silica, net plankton, total plankton, and organic matter.

Although temperature and transparency are of great importance, they do not appear to be limiting factors as far as production of

ponds is concerned. The results also show that pH as such is of relatively little importance either in plankton production or to the fish. Free carbon dioxide may, at the height of photosynthetic activity, momentarily become a limiting factor. This is shown by the fact that on bright sunny days not only is the free CO_2 used up but also a large share of the half-bound CO_2.

Nitrogen is not a limiting factor in the Fairport ponds, since no instance has been found where the water was devoid of free ammonia or of nitrate nitrogen. This means that there is at all times an available supply of inorganic nitrogen for algæ and higher plants. Organic nitrogen was even more abundant, so there was a plentiful supply for the zooplankton.

Since phosphorus has been held to be an important limiting factor by some investigators, it is interesting to find that this is not ordinarily the case in the ponds at Fairport. Dissolved phosphorus does decrease rapidly while plankton is on the increase and at times may disappear completely. A considerable amount of organic phosphorus is always present, however. This organic phosphorus is a source of dissolved phosphorus, which as a rule is reformed quite rapidly. For this reason dissolved phosphorus is never absent for any great length of time. Total absence of dissolved phosphorus has been observed only in ponds that were not fertilized or were fertilized only at long intervals. It thus appears that in unfertilized ponds dissolved phosphorus may at times be a limiting factor, but this is not the case in ponds that are fertilized regularly at frequent intervals.

If dissolved phosphorus is a limiting factor it is certainly not the only chemical constituent that may function in this capacity. In one of the small concrete ponds the dissolved phosphorus dropped from 1.8 to 0.045 p. p. m. during a period of great synthetic activity. The plankton and organic matter increased until the phosphorus reached the lower level, then both declined. Since 0.045 p. p. m. is a relatively large amount of dissolved phosphorus the sudden decrease in plankton and organic matter can not be attributed to lack of an available supply of this element.

In another concrete pond of the same size the amount of organic matter was very high while the dissolved phosphorus was very low. As the season advanced the organic matter decreased and the dissolved phosphorus increased, but this increase in phosphorus was not followed by a corresponding increase in the growth of algæ. The results show very clearly that phosphorus is used by algæ and is again liberated when the algæ die and undergo decay.

These investigations indicate that if the problem of productivity is ever solved it will be found that there is no one limiting factor but that productivity depends on a group of factors, some of which are probably of relatively greater importance than others.

INVESTIGATIONS IN THE UPPER MISSISSIPPI WILD LIFE AND FISH REFUGE

During the summer of 1928 investigations on the ponds and sloughs of the recently established upper Mississippi wild life and fish refuge were begun by Eugene W. Surber under the general direction of Doctor Davis. The inclusion of the bottom lands of

the upper Mississippi in our field of operations has enabled us to widen the scope of our aquicultural investigations very materially. The work at Fairport is necessarily confined to artificial ponds, and it will now be possible to supplement and amplify this work by investigations on fish under natural conditions.

In the past there has been much discussion regarding the feasibility of increasing the production of desirable fish in natural waters, but little real progress has been made in this direction. It is now planned to apply the principles of practical aquiculture to favorable waters in the refuge in an attempt to increase the production of such areas. The periodical overflows to which all ponds in the refuge are more or less liable will no doubt prove a serious handicap to such a program. Certain ponds and sloughs, however, afford much more favorable opportunities for experimental work than others, and it is planned to concentrate on these for the present.

In order to obtain a comprehensive idea of conditions in the refuge Mr. Surber was engaged during the past summer in making an inventory of the ponds and sloughs available for aquicultural investigations. With the aid of a houseboat he was able to cover the most important parts of the refuge and collect data that will be invaluable in developing a comprehensive program for future work.

During June and July, while awaiting completion of the houseboat, observations were made on the fish, bottom fauna, plankton, and physical and chemical factors in a number of typical sloughs. Later an inventory was made of the sloughs that lie in the bottom lands between Lake Pepin and the mouth of the Wisconsin River. The unusually high stage of the river during September and October interfered with the work to some extent but made it possible to cover a much larger territory than would have been possible in a normal season.

The flood plains of the upper Mississippi River are dissected by running-water sloughs or dead-water sloughs, depending upon the stage of the water and alluvial or other obstructions at their origins. These sloughs may be offshoots from tributary rivers or, as is more often the case, offshoots of the main river itself. From the standpoint of aquiculture these sloughs may be classified in five groups as follows:

1. *Running-water chutes.*—These are long, narrow channels, from 20 to 250 feet wide, with abrupt, often undercut, banks. The banks are usually lined with densely foliaged trees of the mature flood-plain forest. Because of strong currents and high turbidity these sloughs are considered unsuitable for fish-cultural purposes.

2. *Quiet-water sloughs.*—Long, narrow sloughs, which were formerly chutes but which now have closing dams or natural alluvial deposits built across their origins. Consequently they have been converted into sluggish sloughs or still-water lagoons with a depth of from 3 to 10 feet. The banks are usually quite abrupt and, due to the deep shade cast by the trees lining the banks, there is ordinarily little aquatic vegetation. Many of these sloughs are considered favorable for aquicultural development.

3. *Short, narrow sloughs.*—These sloughs are usually from about ⅛ to ½ mile in length and from 25 to 200 feet wide, with high banks on both sides sloping rather abruptly into water 5 to 12 feet

deep. A small percentage of the sloughs in this group are oval-shaped ponds. The usual flood-plain forest trees are to be found on their banks. Emergent aquatic plants occur only rarely along their shores, and the zone of submerged vegetation is much restricted. Many of the sloughs in this class appear to be desirable for aquicultural purposes.

4. *Marshy ponds.*—This group includes small ponds, usually from ½ to 4 acres in area. They are characterized by gradually sloping shores and have a maximum depth of 5 to 6 feet. Around the margin of these ponds there is a dense growth of emergent aquatic plants composed of bullrushes, bur reed, and arrowhead, while the deeper parts are covered with submerged vegetation. The surfaces of these ponds are not infrequently covered with floating-leaved aquatic plants such as water lilies and the American lotus. Owing to the dense vegetation in these ponds they can not be utilized for fish-cultural purposes until some practicable method of controlling the aquatic plants has been worked out.

5. *Bottomland lakes.*—The lakes in this group are characterized by their relatively large area. They are usually shallow, and the aquatic vegetation is similar to that of ponds in Group 4 with the addition of *Potamogeton crispus* and wild rice. In the larger, deeper lakes large expanses of open water occur, allowing enough wave action to prevent the growth of arrowhead and weeds along their shores.

Data gathered from about 30 seine hauls made in landlocked or isolated sloughs show that the young of the black crappie (*Pomoxis sparoides*), yellow perch (*Perca flavescens*), bluegill sunfish (*Lepomis incisor*), pumpkin seed (*Lepomis gibbosus*), largemouth bass (*Micropterus salmoides*), golden shiner (*Notemigonus crysoleucas*), common shiner (*Notropis atherinoides*), and spot-tailed minnow (*Notropis hudsonius*) are the most important slough fishes. The young of the black crappie were particularly abundant. Bullheads (*Ameiurus melas* and *A. nebulosus*) are, however, probably more abundant than the catch would indicate, for, like the carp, they burrow into the mud when approached by the seine and thus escape. Very few adult fish were taken in proportion to the number of young, indicating that the larger fish were able to respond to the falling water and return to the main channel or to permanent ponds and lakes leaving their offspring to encounter the vicissitudes of life which these sloughs present in variety.

OYSTER INVESTIGATIONS

Oyster investigations carried out under the direction of Dr. P. S. Galtsoff consisted in the experimental study of oyster culture in New England, North Carolina, and Georgia, in a study of the physiology of the adult and larval oysters, in the survey of natural oyster bottoms, and in an investigation of the biology of the oyster drill.

EXPERIMENTAL STUDIES ON OYSTER CULTURE

Studies on oyster culture were carried out at Onset, Mass., Milford, Conn., Beaufort, N. C., and Doboy Island, Ga. The work consisted in the examination of the conditions controlling spawning and setting

of oysters and in testing the method of collecting spat in wire bags filled with shells. At Onset the field work was done by H. C. McMillin, who made daily temperature and salinity records during the period from June 25 until September 10. To study the distribution and behavior of the oyster larvæ quantitative plankton samples were taken at various stages of tide and in different sections of the bay. A study of setting was greatly facilitated by the fact that the method of planting wire bags filled with shells was adopted by one of the oyster companies, which planted about 1,000 bags over the bars of Onset Harbor. The first oyster larvæ were noticed in the water on July 2, but intensive spawning took place only on July 12. The first set appeared on shells on July 24. Setting was heavy and in certain areas amounted to 69,000 to a bushel of shells.

Special attention was directed to a study of the vertical and horizontal distribution of setting over the bars. A series of wire bags filled with oyster or scallop shells was planted at different levels from 2 feet above low-water mark to 6 feet below it. It has been found that the heaviest setting on all the bars was between mean low-water mark and 1.5 feet above it. No spat were found at the level of high water (3 to 4 feet above low-water mark). Below low-water mark oysters set only in the bag collectors, and the shells scattered over the bottom caught no spat. Bags planted in Shell Point Cove, which is regarded by the oysterman as a body of water unsuitable for planting of shells and where previous attempts to catch spat made by local oystermen were unsuccessful, caught from 6,800 to 7,900 spat per bushel.

The work in Milford Harbor and adjacent waters was carried out by H. F. Prytherch. Under his direction wire bags filled with shells were planted in Milford Harbor by the Connecticut Oyster Farms Co., in New Haven and East Haven Harbors by the Mansfield Oyster Co., and in Branford Harbor by E. Ball & Co. In every locality the plantings were successful, the production of seed oysters ranging from an average of over 15,000 spat per bushel in Milford Harbor to 5,150 spat per bushel in East Haven Harbor. Complete counts of the shells in many of the bags showed that the setting of oyster spat within them had been uniform, and that less than 1 per cent of the shells failed to collect spat. The most intensive setting was found to have occurred in the bags planted just above low-water mark in Milford Harbor. The shells on the bottom of these bags were covered with from 47 to 195 spat each, while those in the top of the collector had from 10 to 35 spat. In this zone the total number of spat collected per bushel of shells ranged from 20,000 to 25,000. Shell samples taken at random over a 10-acre portion of Milford Harbor showed that on this area over 30,000,000 seed oysters had become attached or set on the shells and gravel. The experiments with the shell bags in the several localities mentioned showed clearly that this method of seed-oyster collection can be applied with profit on a commercial scale in inshore areas of this type.

During the reproductive season of oysters in the vicinity of Beaufort, N. C., a series of field experiments was made by Dr. A. E. Hopkins for the purpose of testing methods of spat collecting. Throughout the season wire bags of oyster shells were placed in favorable locations for the collection of spat. Altogether 500

bushels of shells were used and the results studied. Larvæ were found in the water for the first time on May 25, and spat on planted shells were first observed on June 8. After these dates larvæ and spat became more numerous. The peak of the setting period occurred from approximately June 20 to July 15, after which time setting became less and less abundant, although a few new spat were found up to September 1. Natural setting of oyster larvæ in the Beaufort region was very rare for unknown reasons. Because of this the efficiency of the method of collecting spat on oyster shells in wire bags was well demonstrated. Bags of shells left on oyster beds throughout the season collected from three to six spat per shell, depending upon location, while shells scattered over the beds collected a negligible number of spat. Hence, it is apparent that this method is as valuable for North Carolina waters as for waters farther north.

In cooperation with the tidewater commissioner of Georgia, experiments on oyster culture were carried out during June to August by Dr. J. H. Weatherby at Doboy Island. A program similar to that adopted at Onset was followed and consisted in taking temperature and salinity records and in studying the distribution of oyster larvæ. To study the setting of oysters, brush and wire-bag collectors were placed on mud flats and on the bottom of the sound. Few oyster larvæ were found present in the water early in June. On July 4 their number reached the maximum of 400 in 100 gallons of water. A second maximum was noticed on July 20. The first spat were observed on June 10, and setting continued throughout June and July. Heaviest setting took place in the zone between 1 foot below low-water mark and 3 feet above it.

Setting on the wire collectors was fair, varying from 6 to 15 spat per shell, but only occasional spat were found on the brush. The results of the experiments show that the method of using wire-bag collectors can be employed advantageously in Georgia waters.

PREDICTION OF SPAWNING AND SETTING IN LONG ISLAND SOUND

Recent studies have demonstrated that fluctuations in the yield of seed oysters in Connecticut waters can be correlated with variations in hydrographical and climatological conditions. The analysis of these conditions over a period of several years, made by H. F. Prytherch, has shown that the temperature of the water and range of tide were the most important factors in controlling the time of spawning and setting and the production of seed oysters. These investigations proved to be of value not only in understanding certain phenomena in the life history of the oyster but, from a practical standpoint, could be utilized to assist the oyster grower in his cultural operations. This assistance was given in the form of bulletins, which supplied the oystermen with advance information as to (1) whether there would be a crop of seed oysters and how heavy it would be, (2) when the oysters would spawn, and (3) when setting would occur, or, in other words, the date on which shell-planting operations should be completed. These predictions were issued more than a month in advance and for 1928 were as follows:

1. The oysters will be ripe and in spawning condition by July 15.

2. The majority of the oysters on the inshore beds will spawn on or about the 26th of July, while on the offshore grounds spawning at the best will be very light and will occur about August 5.

3. The heaviest setting of the summer will take place about August 10 and will occur chiefly on the inshore areas.

4. The present indications are that the set will be light and slightly better than that obtained in 1923, when conditions were very similar.

Before arriving at these conclusions several cruises were made over the oyster grounds for the purpose of determining the location of spawning beds, the quantity of adult oysters, and the amount of spawn which they contained. Three-year-old oysters were found to be the predominating size and contained approximately one-third as much spawn as was found in the oysters in 1925, when there was a heavy set. Measurements of representative samples of oysters showed that the layer of reproductive tissue in various individuals ranged in thickness from 1 to 5.5 mm. and on the average was 4 mm. thick. The degree of gonad development in 1928, like that of the previous years, could be correlated with the departure of temperature from normal from April to August, which this year was slightly above normal and favorable for a light spawning. The supply of adult oysters on suitable spawning areas was found to be low in comparison with that of previous years and did not serve to increase the chances for obtaining a set.

Generous cooperation was obtained from the Connecticut Oyster Farms Co. and the State shellfish commission in furnishing boats and material for these studies. In order to keep the oyster growers informed as to the trend of conditions on the oyster grounds, bulletins were issued from time to time by the bureau's investigator at Milford, Conn. The bulletins and predictions were turned over to the Connecticut shellfish commission, which distributed copies to the many oyster-growing concerns.

Results of predictions.—The spawning of oysters was predicted to occur on July 26, at the end of spring tides with a new moon. In July the increased range of tide at such times results in considerable heating of the water on a greater area of tidal flats, bringing the temperature to the point (20° C. or above) that is necessary for oyster spawning. This year the water temperature in Milford Harbor increased during this period from 16.5° C. to 22.8° C. on July 26, with the result that harbor oysters spawned on July 23 and oysters on the inshore lots in Long Island Sound on July 26 and 27. The time of spawning was determined by examination of adult oysters, by collection of 24-hour-old larvæ, and was checked by carefully observing the time of setting of the larvæ on planted shells. Setting has been observed to occur on the average 15 days after spawning, when the mean water temperature for this interval is 21.5° C. During the past summer the heaviest setting was found to occur on August 9, 10, and 11, or from 14 to 16 days after spawning, which coincided with the time of setting that was predicted on July 1.

The intensity of setting, as indicated by the number of spat or seed oysters per shell, was such as would be termed a light set. Shell samples that were collected from several lots off Stratford Point and in New Haven Harbor showed an average of 9.5 spat

per shell in the former region and 12 per shell in the latter. Examination of these areas late in the fall showed that a high percentage of the spat had survived and had grown materially.

In summarizing these studies, brief mention should be made as to their application and practical value in the field of oyster culture. In Connecticut and other northern waters the decline of oyster production and the unprofitable operation of oyster farms has resulted largely from repeated failures to obtain a crop of seed oysters on shells planted at considerable expense. An advance knowledge of spawning and setting enables the oyster grower to govern his shell-planting operations so as to obtain the best results at the least expense. On the basis of the predictions for the outlook for each particular year the oyster grower is able to determine (1) the quantity of shells to be planted, so as to take full advantage of the years when heavy setting will occur, (2) how rapidly shell-planting operations must be carried on in order that they may be completed before setting occurs, and (3) the areas or beds that are most favorable for obtaining a set under the existing seasonal conditions.

PHYSIOLOGICAL STUDIES

Spawning.—A study of spawning of the female and male oysters was continued by Doctor Galtsoff at the Woods Hole laboratory. Experiments have demonstrated that the spawning reaction of the female oyster takes place only when the temperature of the water is above 20° C. The experimental evidence is in accord with previous ecological observations, which have shown that there exists a correlation between the rise of the temperature of the water above 20° C. and the appearance of oyster larvæ in the plankton.

Further analysis of spawning reactions carried out under laboratory conditions disclosed the fact that male oysters can be stimulated to discharge sperm by the addition of eggs into the surrounding water. Contrary to what happens with the females, which, after spawning, become for a few days insensitive to the addition of sperm, the reaction in the males can be repeated a number of times, the response occurring immediately after the addition of eggs.

Respiratory exchange in the oyster.—The study of gaseous exchange in the oyster was undertaken by Doctor Galtsoff and D. Whipple as part of an extensive study of the physiology of the oyster and with the hope that it might shed some light on the hitherto unsolved problem of the nature and causes of the green pigmentation of the oyster.

Following the lead of Bruce, two methods were devised for determining the oxygen consumption of oysters under standard conditions. In the first method a water-tight closed chamber was employed, in which six oysters were placed; samples of water were removed at given time intervals and analyzed for oxygen and pH. The difference in the readings showed the oxygen consumption for the time interval and for the total number of oysters used. After a short while it was decided that the muscular activity of the oysters was appreciable and must be controlled. The method was then modified so

that determinations were made on single oysters and a kymograph tracing of the number and extent of the contractions was recorded. Winkler's method for determining oxygen was used, and McClendon's method for estimation of CO_2 from pH and excess base. Using this method the metabolic activities of the oyster under varying conditions were studied. The oxygen consumption of a normal oyster at a temperature of 24.5° C. was found to be approximately 10 cc. oxygen per hour per 10 grams dry weight, the limits being 6.45 to 15.03 cc.

Oysters are relatively insensitive to changes in the amount of oxygen in the water until a level of about 1 cc. per liter is reached. Below this amount a direct correlation exists between the amount of oxygen consumed by the oyster and the amount present. The oyster does not close when the oxygen tension is low but continues to filter water and to consume small but appreciable amounts of oxygen.

The products of metabolism seem to have little effect on oysters. When allowed to remain in a small quantity of water until virtually all the oxygen has been consumed, they come back to a normal rate of metabolism when the oxygen supply is replenished.

Changes in hydrogen-ion concentration produce results similar to those brought about by lowering the oxygen tension. Oysters are not affected by alterations in the H-ion level until a pH of about 6.2 is reached, but below this point there is an appreciable decrease in oxygen consumption. It is of interest to note that at pH 6.0 the buffer action of the shell comes into play, and calcium carbonate is dissolved until the acid-base balance is restored.

The rate of oxygen consumption can be increased by adding to the water readily available food material, for instance, glucose. The rate increases until a certain optimum concentration is reached; if this amount is exceeded the oyster abruptly closes and its exchange with the outside world ceases.

The relation of metabolism to the green pigmentation was not striking. The average value of 5 determinations on white normal oysters was 9.03 cc. oxygen per 10 grams dry weight of oyster, and the average of 6 determinations on deeply pigmented oysters was 11.74. The readings were consistently a few points higher throughout for the green oysters, but whether the slight difference is significant could not be determined.

The copper content of white oysters varied from 8.22 to 13.733 mg. and that of the green oysters from 124 to 272 mg. per 100 grams of dry tissue. The "greening" of oysters still remains a problem for further investigation.

Feeding.—At Beaufort, N. C., and at Woods Hole, Mass., experiments were carried out by Doctor Hopkins on the effect upon oysters of variations in the salt content of seawater. It is know that the water in bays and harbors is subject to considerable change in the relative amount of certain salts. It has been found that a small excess of a salt in the water causes the oyster to close and therefore cease feeding, and also that different salts are very different in their effect on the muscular activity of the oyster. In general, it may be said that in the order of their potency, from high to low, the cation series is as follows: Potassium, ammonium, sodium, lithium, and

magnesium. The anion series falls into the order: Iodides, bromides, nitrates, sulphates, and chlorides. Potassium is of especial importance, for all compounds of it have a highly stimulating influence upon the oyster, causing it to close when only a minute excess of the compound is present. In general the sensitivity of the oyster to chemical changes is remarkable. The oyster's organs for testing seawater are to some substances more sensitive than is the organ of taste in man.

Records of the shell movements of oysters, showing hours spent open feeding and those when closed and unable to feed, have been kept over periods of several months. Contrary to previous observations by Galtsoff an analysis of these records shows a parallel between the brightness of the daylight and the time oysters remain open. The specimens remained open much more during the day than at night. Soon after sunrise the specimens would open if they were closed. Opening occurred most often during the hours of brightest illumination and least often in darkness. It appears that light is one of the factors influencing the feeding habits of oysters. Opening, however, is not due entirely to light, but light is probably only an external factor that has an effect upon the physiology of the organism.

Attachment of the oyster larvæ.—The attachment or setting of full-grown oyster larvæ was not only observed in detail for the first time but was actually produced or imitated under controlled laboratory conditions, and microphotographs were made of each step in the process. Observations made by H. F. Prytherch on the behavior and setting of over 30 larvæ indicate that the swimming or crawling larva first releases a transparent, sticky, fine thread or byssus, which anchors it to the first clean surface with which it comes in contact. The velum or swimming organ is drawn into the shell, and the larva proceeds to crawl rapidly back and forth over the substratum by means of its long muscular foot, from the base of which the byssus is being released constantly. For a period ranging from 3 to 15 minutes the crawling continues but becomes gradually slower and slower until finally the shell is held motionless in an upright position. In the next instant a sticky fluid is secreted by the byssus gland, and the left valve of the larva is brought down into this cementing fluid and held tightly in position against the substratum. The foot and connecting muscles manipulate the shell at this time and hold it in the cement for only a few seconds. The actual period of fixation—that is, from the discharge of the cement to the withdrawal of the foot—is of short duration, rarely lasting over 15 seconds. In one minute from this time the cement was found to be hardened sufficiently to prevent detachment of the newly formed spat by vigorous washing. The orientation of the larva on slides held in a vertical position was always the same—that is, the hinge or dorsal portion of the shell uppermost—while on horizontal surfaces the larva became attached in various positions. Spat on vertical surfaces were found to grow spirally, in a clockwise direction, until the position of the body was just the opposite of that at the time of attachment.

The studies on the attachment of the oyster larva show that setting or the metamorphosis of the larva into a spat is a rapid

process, which, when once initiated, proceeds to its ultimate end regardless of whether or not the larva finds a suitable surface for attachment. For the survival of the organism it is essential, therefore, that the larva come in contact with a surface that is firm and sufficiently clean to insure adhesion of the cementing fluid. The nature of the stimulus necessary to induce setting and its physiological effect upon the oyster larva are at present under investigation.

Response of oyster larvæ to currents of water.—An exact knowledge of the behavior of free-swimming oyster larvæ in currents of water of various velocities is of practical importance in that it may provide an explanation of the "zones of setting" found in many localities and also explain why setting does not occur in certain localities where oysters are known to spawn and larvæ to be present in the water. A preliminary study of this problem was carried out in Milford, Conn., by Dr. R. H. Luce. Two methods of attack were used. The first consisted in finding the distribution of oyster larvæ in a continuous open channel of rectangular cross section in which the water was kept in motion by a paddle wheel. The second method consisted in determining the response of the larvæ to a streamline current of water in a glass tube having a circular cross section.

The diverse results obtained by the two methods may indicate that oyster larvæ respond to currents of water only when there is a sudden and sharp difference in velocity in the water striking the velum and the shell, and that any sudden change in velocity of the water impinging upon the extended velum of an oyster larva may cause withdrawal of the velum, closure of the shell, and a sinking of the larva to the bottom.

SURVEY OF OYSTER BARS OF THE LOWER POTOMAC RIVER

At the request of the Maryland conservation department a survey was made by Doctor Luce in November and December, 1928, of the bottoms of the lower Potomac River for the purpose of determining the extent and condition of the oyster bars in that region. The program in which the Bureau of Fisheries and the State cooperated included the delineation of the principal oyster-producing regions and the determination of the abundance or scarcity of oysters in these regions as the basis for recommendations for greater development of the oyster fishery and the most successful utilization of the oyster grounds.

The area surveyed included about 37 miles of the Potomac River between Persimmon Point on the upper side and Piney Point and Ragged Point on the lower. Ten bars, ranging from 30 to 620 acres in extent, were surveyed on the Maryland side of the river, and six bars, ranging from 60 to 500 acres, were covered on the Virginia side. Some 2,500 acres were covered carefully by the method of triangulation and plotted dredge courses, and in addition large areas were dredged but found to be barren.

The hyrographical and physical data obtained show certain important factors that must be considered in any program of oyster culture in this region:

1. The bars on the Maryland side of the river extend well out into the stream and consist of hard, firm bottom of sand and a mixture of hard mud and sand. The water over these bars ranges from 6 to 30 feet in depth, with an average depth of about 20 feet on the center of the bars.

2. The bottom on the Virginia side of the river is generally mud, which varies from a very soft silty mud in the region of Upper Machodoc Creek to Rosier Creek to hard sticky mud on the section designated "Wakefield." Certain sections on this side of the river were found to be covered with stone about the size of a man's head. The shoals running along Nomini Cliff consist of hard sand ridges with intervening mud sloughs.

3. The bottom from Colonial Beach to Upper Machodoc Creek is liable to shift greatly during northwest storms.

The records of the salinity of the water show that Popes Creek is about the upper limit for the growth of oysters. Certain marked differences in salinity were found between the different sections. A salinity of only 7.12 parts per mille was found off Persimmon Point. Going down the river, the following salinities were found: Lower Cedar Point, 7.97 and 9.16; Swan Point, 8.98 and 10.54; Cobb Point, 10.95 and 11.44; Heron Island, 12.45 and 13.60; Poseys Bluff, 14.38 and 14.61; and Herring Run, 15.15 and 16.37.

The currents were found to be affected largely by the wind. The tidal range is generally less than 3 feet, but in the case of a strong northwest wind the high tide of one day may be lower than the low tide of the previous day. The currents over the bars show a variation from 0 to 20 cms./sec., but the point of observation must be considered for each reading. At Swan Point, for example, there is no flood current near the shore line; the current simply comes to slack and then starts ebbing again. Current observations made on the center of the bar, well out in the river, showed both an ebb and a flood current, with velocities ranging from 3 to 15 cms./sec. At Herring Run Bar, near Piney Point, a strong, downward, eddy current was found at the surface on a flood tide, while slack water was found at the bottom. Is is clear that no general statements can be made concerning the currents setting over the bars. Each bar must be considered by itself, and, should detailed information be desired, must be the object of individual study.

Oyster population on the bars examined.—All of the bars examined were found to be greatly depleted, and, with the exception of Lower Cedar Point and Swan Point bars, all are so denuded of shells and oysters that they must be considered as barren ground. Previous surveys of oyster-producing bottoms have established a minimum of 25 bushels of marketable oysters per acre as indicative of depletion. The greatest density of large oysters in the Potomac was 4.8 bushels per acre at one section of 25 acres at Lower Cedar Point. The other bars had from 0.2 to 1.6 bushels per acre, with an average of about 1 bushel. The condition in regard to small oysters was even worse. Lower Cedar Point and Swan Point bars were the only bars having a sufficient number of small oysters to make an estimate as to the total population possible. These estimates were 3.2, 0.6, 0.7, 0.3, and 0.6 bushels per acre on the sections examined. In regard to spat, it was found that virtually no "set" occurred

during the summer of 1928. Only one section of 25 acres at Lower Cedar Point was sufficiently covered with shells. The other bars were completely stripped of shells, so that a dredge could be dragged as much as 1,000 yards without being filled.

The report submitted to the conservation department of Maryland and published by it contained definite recommendations concerning the program to be followed in the rehabilitation of the oyster beds. These recommendations include (1) the extended closing of the Potomac River to oystermen, (2) the planting of shells and seed oysters, and (3) the establishment of spawning beds of large oysters to aid in producing a set.

OYSTER PROBLEMS OF THE PACIFIC COAST

A preliminary survey of the oyster problem of the Pacific coast was made in October–November by P. S. Galtsoff and H. C. McMillin, who visited the oyster-producing areas in the States of Washington, Oregon, and California. Owing to the general conformation of the coastal waters, the bottoms suitable for oyster culture are very limited and are found chiefly in certain sections of Puget Sound and Willapa Bay. The oyster industry of the Pacific coast deals with three species of oysters—*Ostrea lurida*, *O. virginica*, and *O. gigas*. The first is indigenous to the waters of the Pacific Ocean, while *O. virginica* is imported from the Atlantic coast and *O. gigas* from Japan. Neither Japanese nor Atlantic oysters propagate on the Pacific coast, apparently because of the low temperature of water.

Because the cultivation of the native oyster, *O. lurida*, is of greatest importance from the commercial point of view, it is planned to begin in the new fiscal year a comprehensive study of the biology of this species and to carry out experiments with a view to improving the present method of its cultivation. The work will be undertaken in cooperation with the department of fisheries and game of the State of Washington.

OYSTER-DRILL INVESTIGATIONS

During the past year oyster-drill investigations have been carried on at the Beaufort biological station by Dr. Henry Federighi. The studies, which included experiments on migration, feeding, the effect of lowering salinity, the geotropism and rheotropism of the animal, and on the efficacy of traps, were continued along the same lines as those made at Hampton Roads, Va., in the preceding year. The problem has progressed far enough to permit one to make the following conclusions and recommendations: The drill (*Urosalpinx cinerea*) inhabits hard bottoms (not sandy) wherever oysters are present and wherever the salinity does not fall below 15 parts per mille for any great length of time. It is nonmigratory, depending primarily on the planting operation of man for its distribution. During the winter months the animal does not feed, but as soon as the temperature rises above 15° C. it becomes a dangerous enemy of the oyster, as each drill can kill from 30 to 200 oysters in a single season. The young do not have a free-swimming stage but develop within a leathery capsule, so that they are protected from enemies.

On hatching they immediately attack the oyster spat, causing great damage to the industry. During their months of activity the snails are negatively geotropic and move against the current. Control measures are limited to the following: (1) The use of forks instead of shovels during planting operations; (2) screening of infested oysters, much as sand is screened from gravel; (3) dredging the infested areas with a dredge originally described by Moore (1897) and slightly modified by the investigator; and (4) floating infested oysters for about one week in water of a salinity sufficiently low (about 10 parts per mille) to kill the drills but not to injure the oysters. This last method is undoubtedly the most effective because it also kills the newly hatched drills, which in other methods usually escape.

PACIFIC RAZOR-CLAM INVESTIGATIONS

In 1928 field work on the Pacific razor clam was carried out in April and September by H. C. McMillin. During April commercial canning operations were in progress on the Washington coast and the regular census of the commercial catch was made. The census of 1923 showed a dominance of 6 and 7 year old clams; in 1927 4-year-old clams predominated; and the 1928 census showed a still further reduction in the age of the commercial catch, 2 and 3 year old clams being most numerous. The taking of these small clams constitutes a great economic waste because they are growing very rapidly at this time and have not reached an age when they are important as spawners. The older age groups have disappeared entirely from the beaches, and the entire picture is one of serious depletion.

This depletion has occurred without a reduction in the commercial pack. In fact, the average pack for recent years has shown a slight increase over the period from 1920 to 1924. This case illustrates the danger of relying on the total yield of a fishery as an index of depletion. The decline in the number of clams has been compensated by an increase in the digging effort, the number of diggers having increased more than tenfold during the last 10 years. A crisis in the industry is imminent, as further increase in the digging effort will soon fail to maintain the normal pack and commercial operation will no longer be profitable. Legal restrictions have been proposed that would protect the immature clams and control the catch taken for personal use. The autumn census of the young on the Washington beach showed that a very slight set had occurred during the summer. This fact forecasted still greater need for protection of clams at this place.

Field work in Alaska was suspended for the summer of 1928. Further analysis of data from all localities has been continued by Dr. F. W. Weymouth and H. C. McMillin. It has been proved that all clams upon which growth and other records have been secured belong to one species. Two varieties recognized by conchologists were found to be identical. The characters used to distinguish them were found to change with age or environmental conditions, and no evidence of genetic difference could be discovered. For example, the width of the shell was one character used for separation of the subspecies. It was found that this measurement changed with age in a

remarkable manner. In the very early stages the animal is nearly round; as growth proceeds the shell becomes elongated, until at a length of 4 centimeters the width is about a third of this figure. Further growth is accompanied by an increase in relative width, until at 16 centimeters in length the width is about 43 per cent. The position of the umbo and the thickness of the shells also change with age. Thus, the young from all localities were placed by some conchologists in one subspecies and all old specimens in a different one. Since the life span is short in the southern beds and long in the northern beds casual observations indicated a difference in forms that were thought to be of varietal rank.

Differences between sexes have been found in other mollusks and were sought in the razor clam. Samples of 150 of each sex from Swickshak Beach and Cordova, Alaska, failed to reveal differences, and a like number from Copalis Beach gave similar results. A sample of 115 males and 113 females from Hallo Bay, Alaska, showed a difference in the growth rate between sexes, but other facts make the findings inconclusive.

Analysis of the growth data on the razor clam was continued. A series of growth curves for the same species under 10 environments has been collected. The data for each locality represent the results of the operation of the laws of growth under a certain composite of environmental factors that are in no way artificial. Certain characters of the course of growth are common to all localities, and certain other characters show orderly differences that bear a direct relation to the geographical position of beds.

These data are unique in growth studies, and advantage has been taken of the unusual opportunity to apply the current conceptions of the course of growth, such as those developed by Robertson and by Brody, to this series of curves for a single species grown in different environments. Our data indicate that growth is not an autocatalytic phenomenon, as regarded by Robertson. Brody has claimed that prior to the inflection the growth is in short stages of constant velocity, which pass from one into another by abrupt changes, which are comparable to metamorphosis in insects and other animals. Our data show that this viewpoint is unsound, as no abrupt changes in the rate of growth occur. The concept of the course of growth expressed by Minot seems the most logical from a physiological viewpoint and corresponds most closely to our findings.

The data on the growth of the razor clam and on other forms to which the same method has been applied show that growth follows the compound-interest law, but the rate is constantly decreasing. There is also a body of evidence that indicates that the rate of decrease is constant over long periods of time. The presence of an inflection in the growth curves and other peculiarities can be explained on this basis.

SCALLOP INVESTIGATION

The bay scallop is a mollusk of considerable economic importance, ranking third in value among edible bivalves of the country. In North Carolina, where J. S. Gutsell has made his investigations, it ranks second, is taken commercially in only one county, Carteret, and is of very great local importance.

Of the information obtained in these investigations, the following is worthy of mention here. In North Carolina scallop spawning is primarily autumnal in contrast with more northern areas, where, according to investigations, it occurs in late spring or early summer. As shown by the varying growth and size of scallops on near-by areas, scallops shift little after an early stage. Growth is very rapid and maturity attained in one year. Nearly all are taken before the market season closes in the spring, that is, before the majority are more than 20 months old; only a few survive to be 2 years old. Except for the work of man, the heaviest destructions of adult scallops or of those beyond small size is caused by freshets. In 1927 a rare trematode parasite of the gills was described, and in 1928 a common parasite of the stomach.

At present all attempts at conservation are regulatory. All or nearly all that may be accomplished by such means may be accomplished by a proper closed season beginning some time in the spring before the young scallops are marketable and ending when they have completed spawning. May 1 to January 1 is recommended. If conservation is to be sought by more active means, as some form of scallop culture, it is important to know that scallops shift little and that certain areas produce larger and more valuable scallops than others. If these are depleted they might well be stocked from areas that produce inferior scallops. The quickness with which scallops die out of water offers special difficulties to the planter. On the other hand, the rapid growth, early maturity, and high value offer special inducement.

One report based on these investigations and entitled "Scallop Industry of North Carolina," Bureau of Fisheries Document No. 1043, has been published. Another, embodying the biological studies and provisionally entitled "Natural History of the Bay Scallop," is substantially complete.

FRESH-WATER MUSSEL INVESTIGATIONS

MUSSEL CULTURE

For several years interest in mussel investigations has focused upon the work of Dr. M. M. Ellis, professor of physiology at the University of Missouri, at the Fairport (Iowa) station during the summer and at the university during the college year. Doctor Ellis has been engaged since 1926 in developing a system of mussel culture in which mussel spawn or glochidia undergo, in a nutrient solution, the development that ordinarily occurs while attached to fish as parasites. During the 1927 season, many of the problems that arose in connection with the use of the culture solution were solved successfully, but it was not until 1928 that artificial propagation of mussels on a commercial scale was finally worked out. After Doctor Ellis's return from an extensive European tour in the spring of 1928, six units of mussel-culture apparatus were constructed, each capable of handling at one time upwards of half a million glochidia. The apparatus was set up at Fairport about the first of July, at which time ripe glochidia were scarce. Sufficient were obtained, however, to give the apparatus a thorough and successful trial.

The scarcity of ripe spawn brought to a head another problem. There was found in the marsupia of almost all gravid mussels a protozoan parasite known popularly as Clark's bug, and believed to belong to the genus Conchotherius. This protozoan interfered greatly with the mussel-culture work, for it could not be destroyed as easily as the bacteria also found associated with the glochidia, and when introduced with the glochidia the protozoan increased in numbers rapidly, soon causing the death of all the glochidia. The few gravid mussels available were so infected with the organism that after the test of the mussel-culture apparatus it seemed worth while to make an effort to locate possible sources of gravid mussels free of the protozoan. To this end a trip was made by Doctor Ellis and Mr. Chamberlain in August to mussel waters in a number of States. No glochidia were found that were free of the protozoan parasite.

After returning to the University of Missouri Doctor Ellis continued his studies with the help of several assistants and succeeded in making two valuable discoveries. One was a simplified method of removing from a lot of spawn all glochidia infected with the protozoan parasite. This makes it possible to make use of ripe glochidia as obtained, the infected glochidia being removed and destroyed and the healthy ones utilized.

The second important discovery dealt with the life history of the two species of mussels that have been most extensively propagated by the bureau in the past. These species belong to the genus Lampsilis and include the most valuable of fresh-water mussels, the yellow sandshell, *Lampsilis anodontoides*, and the far more plentiful and, in the aggregate, far more valuable species, the river mucket, *L. ligamentina*. It has been generally believed that the glochidia of these mussels are held in the marsupial pouches of the female in a "ripe" or fully developed state throughout the winter and released, to begin their parasitic life without further development in the spring and early summer. It was found that at least a large proportion of the glochidia are really not quite "ripe" until just before they are released in the spring, even though they are developed enough after the preceding midsummer to attach themselves and live for a time on their host fish. This would seem to indicate that all of the old type of mussel-propagation work that has been conducted after midsummer is of little value.

To meet the situation and prolong the mussel-culture season it is important to find out where the ripe spawn may be obtained earliest in the spring. Presumably this would be in the Southern States. The latest ripe spawn will probably be obtained from such northern waters as Lake Pepin, a widened portion of the Mississippi between Minnesota and Wisconsin, where a valuable species of the genus Lampsilis is found, *L. luteola*, the lake mucket, also called grass mucket and fat mucket.

The immediate plans for mussel culture do not call for additional units of mussel-culture apparatus during the remainder of the fiscal year 1929 because of the likelihood of improving the design in the interest of increased efficiency.

The six units of apparatus now constructed will be returned to Fairport in the early spring and used to full capacity. An esti-

mated production of at least 1,000,000 young mussels is anticipated from these units during the spring period.

A pressing need appears to be to prepare for the intelligent distribution of the young mussels after they are produced. Since the publication of statements regarding the development of the bureau's mussel-culture investigations many requests have been received for allotments of young mussels. A knowledge of the factors favorable to the growth of mussels and the condition of the waters to be stocked is essential for the choice of the most suitable species for each unit of water area. Hence, an extensive study of many waters is planned for the coming summer to assist in drawing up a list of requirements for the use of the bureau to be met by all applicants for young mussels.

MUSSEL SURVEYS

Two mussel surveys were made during the summer of 1929. The first was at Lake Pokegama, Minn., during July, and the second was at Lake Pepin in August. Both bodies of water have been large producers of mussels of value to the pearl-button industry in past years. Also, at both lakes the bureau has conducted mussel propagation work for many years.

During the summers of 1920, 1922, 1923, 1924, and 1925 mussel suveys were made in various bodies of mussel-producing water by the Fairport station, but in each case the crowfoot rig was used for collecting the mussels, and this method is considered inaccurate for such detailed studies.

It was apparent that accuracy could be expected only from some appliance that would show all the mussels present in a given unit area, or at least all the mussels in this unit area not entirely submerged in the bottom material. To this end, T. K. Chamberlain, director of the Fairport station, spent a portion of the summer of 1927 working upon such appliances and finally developed a clam-shell type of bottom dredge that would remove from a sand, gravel, or mud bottom all adult commercial mussels over 6 square feet of surface, which were not entirely submerged in the bottom material. This would include all those mussels that might, under the most ideal conditions, have been taken by the crowfoot rig. The dredge was found to have some obvious deficiencies and would not work on stony bottom or a bottom having much wood débris. On the other hand, a crowfoot rig would not work in such places either, so it may be stated that the dredge would work on any bottom where a crowfoot rig might be used and would give more accurate results.

The dredge was used on both Lake Pokegama and Lake Pepin. Lake Pokegama had been closed to shelling for 11 years, during which time the bureau had inoculated fish in the lake with 1,000,000,000 glochidia at a total cost of $15,000. It was found that the lake had not recovered its mussel population to the degree to be expected from the amount of propagation work done. A study of the growth of the mussels from the lake by T. K. Chamberlain also showed the rate to be unusually slow. The quality of the shell was also much inferior to what it had been 15 years earlier. The full responsibility for the adverse situation is attributed to the unfavorable chemical changes in the lake through the changing of the course of the Snake

River, a mussel-producing stream that formerly flowed through the lake. About 12 years ago this river was given a new channel around the lake instead of through it, leaving a channel that is insufficient for proper exchange between the two bodies of water.

The Lake Pepin survey conducted by H. O. Hesen and H. C. Minch showed that the lake has been subjected to heavy shelling but is still the source of an extensive commercial mussel population. It seems to be a desirable locality for the planting of juvenile lake muckets.

GROWTH STUDIES

For the past two years T. K. Chamberlain has been engaged in studies on the rates of growth of mussels from various localities with a view to using this information as an aid in determining where to plant to greatest advantage the young mussels to be produced by the bureau's new mussel-culture system.

Up to this time actual growth determinations have been made for the most valuable of all fresh-water mussels, the yellow sandshell, *Lampsilis anodontoides*, as found in the Mississippi River in the vicinity of Fairport and as found in the White River of Arkansas; for the lake mucket, *L. luteola*, as found in Lake Pokegama, Lake Cross, and Lake Pepin, while at present growth measurements are being made on the river mucket, *L. ligamentina*, as found in Fox River and in the St. Joseph River.

DIAMOND-BACK TERRAPIN CULTURE

Experiments in terrapin culture at the fisheries' biological laboratory at Beaufort, N. C., under the direction of Dr. S. F. Hildebrand, except for an epidemic of sores among the brood of 1927, progressed satisfactorily. The hatch increased from 6,887 in 1927 to 9,095 in 1928.

The epidemic of sores referred to occurred among both the winter-fed and the hibernating animals of the brood of 1927. While the disease is not new, it previously seldom had reached epidemic proportions. With the hope of gaining some information about this disease and possibly finding a preventative or cure, the services of Dr. Herbert Fox, a pathologist of the University of Pennsylvania, were secured. Doctor Fox soon found the disease to be a complicated one, and during the limited time that he could devote to its study he was unable to develop a preventative or a cure. The disease may attack any part of the animal. It most often attacks the tail, however, and it occurs not infrequently underneath the plastron, being visible externally as pale or yellowish areas on the shell. The earliest symptom of the disease is the appearance of a pale spot on the skin. The skin in such a place soon sloughs off and a sore covered with a "scab" results. It is for this reason that the disease has been termed "sores." The sore gradually eats into the tissues attacked. On the tail, for example, it generally starts at or near the tip, with the result that the member is gradually destroyed. When the gradual deterioration reaches the vent, or near that point, death invariably results. Sometimes, although not often, the sores heal after a part of the tail has disappeared, and the

animal recovers. When the disease attacks the feet or legs it is much less destructive. Plastron cases are less numerous than tail cases but apparently equally fatal. This disease is particularly annoying because it most frequently attacks the largest and most rapidly growing animals.

In the brood of 1927, among 3,730 winter-fed animals, a loss of 13.4 per cent was ascribed to sores. A further loss of 15 per cent was ascribed to "soft shell," which was rather lower than the usual loss from this source. Soft shell is a condition correlated with a failure to feed, resulting in emaciation and often in death. Soft shell occurs almost exclusively among terrapins that probably never have taken food and never have gained growth.

During the year 2,323 young terrapins that had gained considerable growth and had passed through the most critical stage of life were liberated.

A new terrapin house 65 feet long and 20 feet wide was built during the year. This house is provided with a steam-heating plant, making it possible to provide a more uniform heat than was possible previously when a stove was used. The brood of 1928 is housed in this building, and to date (January 16, 1929) there is no indication of a recurrence of last year's epidemic of "sores," although a few cases are present.

A general account of the many experiments in diamond-back terrapin culture that have been conducted at Beaufort, showing the results achieved to date, was prepared during the year and is in course of publication.

STUDIES ON INTERIOR LAKES

In the summer of 1928 the bureau's investigation of growth rates of fishes in Wisconsin lakes by Dr. Stilman Wright was continued with the collection of data and scales from 5,000 fish of several species. Field collections were made by J. B. Goldsmith and C. E. Juday, temporary assistants. For the present work is being restricted to a study of the scale material of 1,200 ciscos taken from eight lakes. A preliminary study of a small series of scales from each lake shows unquestioned evidence of marked differences in the rates of growth in different lakes, but in most cases the data must be considered inadequate to serve as a final basis of comparison. It seems probable that several hundred individuals will be needed from each lake for final results. If the data at hand appear to justify it, collecting will be continued and extended in the summer of 1929.

The bureau again cooperated with the State geological and natural history survey in limnological studies of the lakes of northeastern Wisconsin. Buildings were provided by the State conservation commission on the shores of Trout Lake, and chemical and other laboratory equipment was installed.

The first observations on the lakes were made on June 22 and the last on August 29. Between these dates observations were made on 227 different lakes, 85 of which were visited for the first time, the other 142 having been visited one or more times in previous years.

The field party consisted of E. A. Birge, C. Juday, J. R. Hickman, E. B. Ruth, and Hugo C. Baum, biologists, and Rex J. Robinson, Lloyd Setter, and W. H. Woodstock, chemists. B. L. Browning,

F. H. L. Taylor, and C. H. Winning made chemical analyses of the residues that were sent to the chemical laboratory of the University of Wisconsin.

The chemical and biological work followed the same lines as indicated in the reports for 1926 and 1927 for the most part. A more extensive quantitative study of the bottom fauna was begun in 1928 and the results obtained will be considered in a subsequent paragraph.

As indicated in previous reports, the field studies included such physical items as the temperature, transparency, color, and conductivity of the water, as well as the rate at which the water cuts off or absorbs the sun's energy. The chemical items included hydrogen-ion concentration, free and fixed carbon dioxide, dissolved oxygen, free ammonia, nitrite, nitrate, and organic nitrogen, soluble and organic phosphorus, silica, and chlorides. The biological phases of the work consisted of quantitative studies of the plankton and of the microscopic bottom fauna. Several large samples of centrifuge plankton and of townet catches were secured for chemical anaylses.

During the summer about 550 hydrogen-ion determinations were made and a somewhat larger number of free and fixed carbon dioxide and dissolved oxygen determinations. More than 400 determinations of nitrogen compounds, phosphorus, silica, and chlorides were made, and a similar number of determinations of the dry organic matter in the centrifuge plankton. Also, 415 residues were obtained by evaporating 2 to 5 liters of water; these residues are being used for more complete chemical analyses according to micro methods that have been used for the past four years. Two research assistants appointed by the University of Wisconsin are now engaged in determining the quantity of organic nitrogen, organic carbon, ether extract, and sulphate-ion in these residues. A third chemist is working out field methods for the rapid determination of calcium, magnesium, and sulphate ions.

As indicated in previous reports, these lakes show a wide range in the amount of mineral and organic matter that their waters hold in solution. The amount of dry residue in surface samples, for example, ranged from a minimum of about 9 mgm. to a maximum of 107 mgm. per liter. The hydrogen-ion varied from pH 5.4 to pH 9.1. The fixed carbon dioxide ranged from 0.2 to 29.0 mgm. per liter; in the great majority of the lakes the quantity fell between 1 and 15 mgm. per liter. Free ammonia and nitrite nitrogen were rarely found in the surface water, and then only in traces. The nitrate nitrogen varied from 0.008 to 0.050 mgm. per liter and the organic nitrogen from 0.13 to 1.36 mgm. The surface water of a few of the lakes contained no soluble phosphorus, or only a trace, while a maximum of 0.012 mgm. per liter was noted in two lakes; in most instances the quantity ranged from 0.003 to 0.006 mgm. The organic phosphorus varied from a minimum of 0.007 to a maximum of 0.085, a twelvefold difference.

The quantity of dry organic matter in the centrifuge plankton of the surface water varied from 0.28 to 6.36 mgm. per liter, which is almost a twenty-three fold difference. In general, the soft-water lakes yielded a smaller amount of plankton than those that had a larger amount of electrolytes in solution.

During the summer two of the biologists devoted their whole time to a quantitative study of the larger organisms that live upon and in the lake bottom. They obtained something over 1,200 samples from 51 different lakes. The largest number of samples was obtained from Trout Lake; in general the number of samples taken in the other lakes varied from 6 up to 25, depending upon the size and depth of the lakes. The bottom population consisted primarily of insect larvæ and amphipods. A few of the samples did not contain any of the larger organisms, while a maximum of 8,900 insect larvæ per square meter was obtained in one lake; the latter represents a dry weight of 167 pounds per acre.

FISHERIES BIOLOGICAL LABORATORIES AND FIELD STATIONS

The bureau's fisheries biological laboratories at Woods Hole, Mass., Beaufort, N. C., and Fairport, Iowa, were occupied and operated during the entire year of 1928, although only the latter two have permanent directors. In addition to these, laboratory quarters of more or less temporary character have been furnished to field units of the bureau's scientific staff by the College of Fisheries, University of Washington, at Seattle; the museum, University of Michigan, at Ann Arbor; the natural history museum at Stanford University, Calif.; and the Museum of Comparative Zoology of Harvard University at Cambridge, Mass.

The fisheries laboratory at Woods Hole, Mass., served as headquarters for several investigators of the Middle Atlantic staff during the winter and was opened to private investigators for the summer season from June 20 to September 15 under the directorship of Elmer Higgins, in charge of the division of scientific inquiry. During this period the oyster investigations under Dr. P. S. Galtsoff, the mackerel investigations under O. E. Sette, and the Middle Atlantic fishery investigations under R. A. Nesbit were centered here, all of which have been discussed already. Important studies of the physiology of respiration in fishes were also continued by Dr. F. G. Hall of Duke University and Dr. Irving E. Gray of Tulane University.

In adition to the bureau's staff, a number of independent investigators were engaged on various problems of general biological significance. Dr. N. A. Cobb, of the Department of Agriculture, with a staff of six assistants, continued his studies on nematodes. Three of the university tables were occupied—Harvard by H. E. Bowen, Johns Hopkins by Dr. John C. Hemmeter, and Princeton by Samuel E. Hill. Space was assigned Dr. Edwin Linton, University of Pennsylvania; Dr. Attilio Rizzolo, national research fellow; Paul Conger, diatomist of the Carnegie Institution; Dr. Howard B. Stough, University of Idaho; and Miss Ruth Schwartz of the University of Colorado.

Numerous repairs to the laboratory building and residence were made during the season. A new laboratory room for biochemistry was equipped; an oceanographical laboratory room was refinished, and the storeroom entirely rebuilt and completely equipped, all of which materially increased the efficiency of the laboratory.

During the summer season a series of special lectures was presented to large and appreciative audiences of investigators, and motion pic-

tures of fisheries and bureau work were shown on two occasions. The lecturers were Dr. P. S. Galtsoff on "The chemistry of the sea"; Dr. F. G. Hall on "Respiration in fishes"; Dr. A. G. Huntsman, director of the Atlantic Biological Station, St. Andrews, N. B., on "Limiting factors in the sea"; and Dr. H. B. Bigelow, Harvard University, on "Oceanography and fisheries."

At the invitation of the Bureau of Fisheries, the National Association of Fishery Commissioners held its annual meeting at the Woods Hole biological station on September 7 and 8. The program presented was unique in many ways, for it consisted primarily of an elaborate demonstration of the bureau's scientific investigations in oyster culture.

Among those who addressed the convention were Col. Harry Houston, the president of the association, Congressman Charles W. Gifford, Lewis Radcliffe, and Dr. P. S. Galtsoff. Motion pictures of the industry were shown by Howard W. Beach.

The main laboratory of the fisheries station was given over to exhibits illustrating the oyster investigations, in which live animals were utilized as far as possible. The exhibits were arranged under subject headings, so as to tell a complete story. Printed descriptions were posted on each table, and, in addition, members of the staff acted as guides in explaining the significance of the experiments and how to operate the apparatus. The following exhibits were presented:

FEEDING

1. Apparatus showing the current produced by the oyster gills.
2. Effect of temperature on the function of gills; cessation of feeding caused by low temperature.
3. Ciliary movement of the gill epithelium; motion of food particles along the surface of the gill.
4. Ciliary motion under microscope.
5. Transverse section of the gill.
6. Apparatus for collecting the water after it has passed through the oyster gills and for measuring the pressure inside the gill cavity.
7. Charts and diagrams showing the effect of temperature on the amount of water passed through the gills.
8. Diagram showing chemical sensitivity of the oyster.

OPENING AND CLOSING OF SHELLS

1. Apparatus used for recording the opening and closing of the shell.
2. Diagram showing the frequency distribution of the periods of opening.

SPAWNING

1. Apparatus used for studying the spawning of female oysters, consisting in constant-temperature bath, kymograph, electric clock, stirring mechanism, and æration mechanism.
2. Series of kymograph records showing how spawning reaction in the female oyster can be produced by the addition of a small amount of sperm to the water.

GREEN OYSTERS

1. Specimens of green oysters from Long Island Sound.
2. Blood cells containing copper.
3. Apparatus for studying the metabolism of the oyster.

OYSTER CULTURE

1. Spat collector (wooden crate) from Onset containing 75,000 spat to a bushel.
2. Wire bag filled with oyster shells and used as spat collector in Wareham River and Onset, Mass.
3. A series of spat on oyster shells, 1 to 42 days old, taken from experimental oyster ground at Onset.
4. Live spat attached to glass microscope slides.
5. Series of diagrams showing setting conditions in various waters and probable effect of tides on distribution of setting zones.
6. Bulletins issued in June predicting time and intensity of spawning and setting in Long Island Sound.
7. Diagrams and charts explaining the method of predicting spawning and setting.

ANATOMY OF THE OYSTER

1. Series of sections through various organs of the oyster, showing gills, digestive tract, development of gonads, structure of mantle, and tentacles.

OYSTER DRILL

1. Development of the drill and its growth.
2. Boring mechanism of oyster drill.
3. Specimens of shells bored by the drill.
4. Charts showing time required to bore various shells.
5. Specimens of live drills.
6. Outline of measures for combating drill.

In addition to the above exhibits, aquaria containing specimens of various shellfish that live in the vicinity of Woods Hole and their principal enemies were displayed. The following exhibits of the Pease Laboratory (Inc.), of New York, were shown:

1. Charts and diagrams showing bacteriological examinations of oysters at Hampton Bar, Va., and in Rhode Island.
2. Charts and kymograph records showing the effect of chlorine on opening and closing of the shell and on ciliary activity of the oyster gill.

Approximately 75 persons attended the sessions of the convention, including State fishery or shellfish commissioners of Massachusetts, Connecticut, Maryland, and Virginia; public health officers of these and other States; private investigators; and leading oyster producers and dealers.

The work of the Beaufort (N. C.) biological station is mentioned elsewhere in this report in connection with investigations pertaining to young fish, oysters, scallops, and diamond-back terrapin culture. The regular staff of the station was augmented by the addition of one boatman, who has charge of a new 46-foot, 2-cabin type cruiser secured during the year. The working facilities of the station were increased further by many improvements and repairs and some new equipment. Notable among the improvements perfected or under way to January 1 are the construction of a new terrapin brooder house, described eleswhere, the installation of a central heating plant, new electric wiring and fixtures throughout, and new and additional plumbing.

The collection and the study of young fish, mentioned elsewhere in this report, were carried on by Dr. Samuel F. Hildebrand, the director, assisted by James S. Gutsell and Louella E. Cable. Doctor Hildebrand, assisted by Charles Hatsell, the foreman, also carried on a large series of experiments in diamond-back terrapin cul-

ture, of which a more extended account occurs elsewhere. James S. Gutsell continued and virtually brought to a close an investigation on the bay scallop begun in the summer of 1925. Louella E. Cable conducted some scale studies of the pigfish. Drs. A. E. Hopkins and Henry Federighi carried on investigations pertaining to problems in oyster culture during the greater part of the year.

In addition to the investigations already mentioned and carried on by the bureau's staff, 24 other workers, mostly private investigators, visited the station during the course of the year.

Experiments pertaining to wood preservation and ship-bottom fouling, conducted by the Chemical Warfare Service at this station for several years, were continued and extended under the direction of H. W. Walker of that service. Bernard Gehauf, of the same service, performed an extended series of tests relative to the toxicities of ship-bottom paints on larval barnacles.

Prof. H. V. Wilson, of the University of North Carolina, assisted by James T. Penny, studied the cell behavior in the regeneration of sponges from dissociated cells.

Dr. Bartgis McGlone, of the University of Pennsylvania, assisted by Charles B. Tatnall, continued his studies on the effects of hydrogen-ion concentration of sea water on the fertilization of marine eggs. A similar line of studies was undertaken by Dr. Ferdinand A. Ries, of the University of Maryland, assisted by Leo C. Drummy. However, owing to an illness suffered by Doctor Ries, virtually no headway was made by the last-mentioned investigators.

Prof. A. S. Pearse, of Duke University, and assistants made some preliminary studies of the ecology of brackish-water animals, using pricipally crabs, of which 10 different species from different habitats were studied for evidences and methods of and reasons for migrations from sea to land and fresh water.

Dr. C. W. George, of the University of North Carolina, was engaged principally in writing and revising a manuscript on some work done on the blood cells of ascidians during previous years at the station.

A. S. Rose, of the University of North Carolina, a student under Doctor George, spent several weeks at the station studying under his professor's supervision the structure and classification of local fishes.

Dr. E. P. Churchill, of the University of South Dakota, devoted his attention to the study of the blue crab, principally the juvenile stages, which remained in part undescribed.

Dr. Ellinor H. Behre, of the University of Louisiana, assisted by a student, Lillian O. Henderson, continued studies of two previous summers on the effect of light on fishes.

Dr. Hoyt S. Hopkins, of New York University, carried on some studies on the respiration and contractibility of muscles in clams.

Dr. H. W. Manter, of the University of Nebraska, examined 303 fishes belonging to 50 different species for parasites, principally for trematodes. These parasites were found in 37 of the 50 species examined and were retained by Doctor Manter for further study.

Others who spent several days at the station are Prof. Duncan S. Johnson, of Johns Hopkins University, assisted by Duncan S. Johnson, jr., who collected some plants on the "banks"; Dr. Waldo Schmitt and Clarence Shoemaker of the National Museum, who col-

lected crustaceans; and B. C. Indrambarya, Siamese Government student, who also collected crustaceans and who came to obtain first-hand information concerning the means and methods employed in diamond-back terrapin culture.

During the calendar year of 1928 the regular scientific staff of the fisheries biological laboratory at Fairport, Iowa, consisted of the director, T. K. Chamberlain, who, in addition to the administrative duties of the position, had immediate charge of the fresh-water mussel investigations, and A. H. Wiebe, who conducted the pond-culture studies referred to elsewhere. During part of the year E. W. Surber made the station his headquarters, and during the summer several temporary scientific assistants were employed. The work of the station has been fully described elsewhere in this report under the heads of mussel culture and pond culture.

The station at Key West, Fla., remained inactive during the year pending its final return to the original owners of the property.

Part II.—DIVISIONAL CONFERENCE, JANUARY 2 TO 5, 1929

THE CONFERENCE

The division of scientific inquiry held its second general biennial conference on January 2 to 5, 1929. In addition to the bureau's staff of about 50 investigators, including both the departmental and the field service, a considerable number of well-known aquatic biologists and fishery officials from universities, State commissions, and commercial organizations were in attendance.

The first two days of the conference were devoted to the presentation of papers upon the more general phases of fishery biology and its applications; during the last two days the conference divided into three sections for discussions of technical details of the bureau's various investigations. One of these groups considered commercial fishery investigations, another fresh-water fisheries and aquiculture, and the third shellfish problems.

The sessions on January 2 and 3 were held in the National Museum and the section meetings on January 4 and 5 at the Bureau of Fisheries. The following program was presented:

Wednesday, January 2

1. Opening address. Henry O'Malley, Commissioner of Fisheries.
2. Purposes of the conference. Elmer Higgins, in charge, scientific inquiry.
3. A review of the fishery situation. Lewis Radcliffe, deputy commissioner.
4. What biology may do for the commercial fisheries. H. F. Taylor, vice president for research, Atlantic Coast Fisheries Co.
5. Progress of fishery biology on the Atlantic coast. O. E. Sette.
6. The Passamaquody project and the fisheries. Dr. H. B. Bigelow, Harvard University.
7. Studies on respiration in fishes. Dr. F. G. Hall, Duke University.
8. Fishery conservation in California. Dr. J. O. Snyder, Stanford University.
9. The international halibut investigation. W. F. Thompson, International Fisheries Commission.
10. Progress of fishery biology on the Pacific coast. Dr. Willis H. Rich.

Thursday, January 3

1. Biology and the cultivation of shellfish. Dr. P. S. Galtsoff.
2. Artificial culture of glochidia of fresh-water mussels. Dr. M. M. Ellis, University of Missouri.
3. Progress of fishery biology on the Great Lakes. Dr. John Van Oosten.
4. Investigation of Wisconsin Lakes. Prof. Chancey Juday, University of Wisconsin.
5. Cooperative Lake Erie investigations. Dr. C. J. Fish, director, Buffalo Museum of Science.
6. Some fresh-water sport-fishery problems. Seth E. Gordon, conservation director, Izaak Walton League of America.
7. Progress in fish cultural investigations, Dr. H. S. Davis.

Commercial fisheries section. January 4 and 5

Dr. Willis H. Rich, presiding.

Salmon Investigations. H. B. Holmes, Columbia and Chignik Rivers; A. C. Taft, Karluk River.

Pacific herring investigations. Geo. C. Rounsefell.

Pacific halibut. W. A. Dunlop, International Fisheries Commission.

Great Lakes investigations. Dr. John Van Oosten, "Fishery situation in Lake Erie"; H. J. Deason, "The pike-perches"; Ben H. Hill, "Effects of fishing gear."

North Atlantic investigations. Dr. Wm. C. Kendall, "Smelt"; Wm. C. Schroeder, "Cod"; O. E. Sette and E. W. Bailey, "Mackerel."

Middle Atlantic investigations. R. A. Nesbit, "Squeteague"; H. A. Hanson, "Statistics of yield."

South Atlantic investigations. Dr. S. F. Hildebrand, "Larval fishes"; Isaac Ginsburg, "Texas fishes."

Aquiculture section. January 4

Dr. H. S. Davis, presiding.

Prof. G. C. Embody. Stocking fresh waters.

M. C. James. Recent developments in the division of fish culture.

R. F. Lord. Trout culture.

A. H. Wiebe. Pond culture.

Eugene W. Surber. Upper Mississippi survey.

Dr. Stillman Wright. Growth of fish in Wisconsin lakes.

Shellfish section. January 4

Dr. P. S. Galtsoff, presiding.

Dr. H. D. Pease. Scientific work of the Oyster Dealers and Producers Association.

Prof. R. V. Truitt. Oyster investigations in Maryland.

H. F. Prytherch. Oyster studies at Milford, Conn.

Dr. A. E. Hopkins. Physiological studies.

H. C. McMillin. Pacific coast oyster problems.

Dr. R. H. Luce. Oyster survey of the lower Potomac.

Dr. Henry Federighi. Oyster-drill control.

J. S. Gutsell. North Carolina scallops.

T. K. Chamberlain. Fresh-water mussels.

The following papers are selected from the first two sessions of the conference. Unfortunately not all the addresses can be published. Some were not presented from prepared manuscripts and others were too technical or detailed to be suitable for this report. Because of the comprehensive treatment of the subjects here presented it is believed that the general reader will gain a better understanding of the recent work of the division of scientific inquiry and its relation to fisheries biology than could readily be obtained otherwise.

OPENING ADDRESS

HENRY O'MALLEY, *Commissioner of Fisheries*

I am very glad indeed to welcome you here again for the second general conference of the division of inquiry. I am especially happy to greet the new members, most of whom represent additions to our scientific staff rather than replacements. I am also glad that there are so many of the bureau's friends and associates here who are interested in fisheries conservation, although not officially connected with the bureau. Some may even be regarded as alumni of the bureau—former members of the staff who have received training in our organization and have graduated to larger fields of endeavor.

It was remarked, following the conference of two years ago, that the undertaking was an experiment, but that it could hardly be doubted that the efficiency of the bureau's work would be increased materially by overcoming the isolation of the field investigators, by bringing them into contact with workers in associated fields and by permitting the exchange of ideas and criticisms. Results of the first conference have been so favorable that it has been deemed worth while to repeat the experiment this year. Hence we are assembling again to consider anew the objectives for which we are striving, the progress which we have made since the last meeting, and the means by which we may improve our methods of attaining our goal.

The ultimate objectives of the bureau's work have changed but little since the establishment of the bureau 57 years ago, and they are so well known that they scarcely need repetition; but the methods of realizing these aims have changed and developed along with the growth of biological knowledge and with changes in material circumstances. There may have been times in the past when the bureau failed to appreciate the magnitude of the forest because of the density of the trees in its path, but we are now again in open country, and I am confident that real progress is being made toward utilizing the nation's fishery resources to their fullest extent without encroaching upon the supply of the future—in short, toward real conservation.

I shall not attempt to outline for you the progress made during the past few years. It is *my* job to direct your policies. It is *your* job to develop and apply the methods of successful investigation. That your work is good is evidenced by the increasing confidence of the public and the industry in the results of the bureau's investigations and dependence on our recommendations.

In my last official report I stated that the enlivening of the public conscience to the need for proper husbandry of our fishery resources is the most heartening feature of the present fishery situation. Coincident with the rapid development of the commercial fisheries of the country is the growth of a greater appreciation of the work of the scientist in placing fisheries research on a more practical basis. The fisheries biologist has demonstrated the value of such research and has thereby created a greater demand for his services. But fishery science is rapidly developing. Fads and enthusiasms will inevitably arise. Hence, I feel moved to warn you, as pioneers in a new field of scientific research, not to lose sight of your main objective or become engrossed so deeply in the development of technical methods or abstract principles that you forget the practical applications. It is only by continued application of this newly acquired knowledge of fishery biology to the practical problems of fishery administration and development that the confidence and support of the public can be maintained.

A convincing evidence of public approval is the increased cooperation on the part of State and private organizations in fishery investigations. Demands for investigations still exceed the facilities of the bureau, but the bureau's effectiveness has been materially extended through the close association of its own trained investigators with the State commissions. Few States have the resources or the organization to maintain a staff of biologists. They have accordingly furnished boats, men, gear, and funds for cooperative surveys and experiments, and at our suggestion several States are actively engaged in the collection of fishery statistics that are sufficiently detailed and extensive to be useful to the biologist in studying abundance. This increased activity has brought our investigators into closer contact with the local officials, and matters of administrative and even political nature have been forced upon their attention.

While all of you are specialists and are deeply engrossed in your individual scientific work, I urge you to report every unanswered problem encountered,

together with your recommendations for work in new fields or along new lines. It is our duty to be far in advance of the actual needs of the industry, to anticipate its problems, and to be ready with helpful suggestions. The only way that the bureau can maintain close contact with the fishing industry is through its field staff, and I urge upon you the importance of being alert to every opportunity for service.

This conference is intended to give each of you an opportunity not only o review and criticize the work of others but to examine your own task, to defend your own plan of work, and to assess the value of its results. I hope that no false modesty or real shyness will prevent you from entering freely into the discussions. Let no question remain unanswered and no statement of doubtful principle or fact go unchallenged. The bureau's program of work must be examined and overhauled in the light of the present and future needs of the fisheries; all weaknesses repaired; all faulty timber removed.

While the bureau's policy must be announced by its official head, it must be largely determined by the combined judgment of its entire staff. *You* are the Bureau of Fisheries, not the commissioner's office or the offices of chiefs of divisions. The results of your whole-hearted cooperation during the past two years have been very gratifying. We now have before us the responsibility of planning more wisely and executing more efficiently and more faithfully.

PURPOSES OF THE CONFERENCE

ELMER HIGGINS, *In charge, Division of Scientific Inquiry*

I shall not burden you with a long recital of the purposes of this meeting. The commissioner has already told you that we have met again to reexamine the objectives of our investigations and to review the progress we have made in realizing them.

I have heard many comments, particularly from the younger men, concerning the inspiration received from the last conference, and because of our belief in the real benefits derived by all of you from rubbing shoulders, comparing notes, and matching wits with the other fellow we have planned this second conference. I sincerely hope that the discussions during the next few days will be of even more benefit than before. It appears from the number present that it will be of benefit to a greater number at least. This conference will certainly be worth while if the isolation of the field workers can be overcome in any degree. Everyone benefits from more intimate contact, but much of the benefit will be lost if the contacts made at this meeting are allowed to lapse. Certainly everyone can receive help from someone else on the staff having similar problems, and I hope that many of you may feel free to seek this help by correspondence throughout the year. I suspect that many of us have overlooked this source of effective and expert aid.

As will be seen from the program, the conference will be divided into two parts; the first part is intended to acquaint us with the outstanding work in fisheries biology during the past two years; the second half of the program is intended to review the individual programs of the various investigators. Our organization is now so large that it is difficult to permit full discussion of the many undertakings in a single general meeting. The program would be too long or else many would be neglected if such a plan were followed. The first part of the program will, therefore, by synoptic and general dealing with aims and results of investigations, with what has been studied and what should be studied; the second part should be detailed and exhaustive, permitting free and full discussion of methods and means. I do not propose to limit discussion to-day or to-morrow any more than is absolutely necessary to complete the program within the allotted time. In the section meetings reports of about 15 minutes' duration have been requested from each investigator, but these are only intended to form the basis for as much discussion as is desirable; and if it appears worth while, the sections can be continued even into next week.

I know that none of you wish to become involved in Government red tape any more than is absolutely necessary, but I believe that you will be interested in a brief review of some phases of the division's business during the past two years. The funds for the continuation of the work of the division of scientific inquiry are furnished by seven different appropriations, totaling for the present fiscal year over $243,000; and, in addition, a special appropriation of $20,000 has been provided for construction, repair, and improvement at the fisheries biological laboratory at Beaufort, N. C. Aside from this special fund, only

two of these appropriations are used exclusively by the division of inquiry, hence the figures are given in round numbers and the amount varies from year to year.

The commissioner has spoken of the evidences of increased approval on the part of the public of the scientific work, but so far as we investigators are concerned this approval can be gauged only by the funds Congress provides. Judging by this standard, there is no doubt of popular sanction, for the appropriations have more than doubled in the past five years. Considering the two appropriations, "Inquiry Respecting Food Fishes" and the combined salary rolls of inquiry, the annual totals from 1924 to 1929 are as follows: $91,000, $118,000, $129,000, $144,000, $175,000, $208,000. Besides these funds, the amount for the operation of vessels for scientific work has also been increased in accordance with the demand. These have permitted the purchase of a new motor vessel and a small launch for the Beaufort laboratory and extensive repairs and alterations on the *Albatross II* for North Atlantic work and on the *Shearwater* for Lake Erie work, better fitting them for fishery investigations, as well as providing and repairing boats and launches at the stations. The salary appropriations since 1924 have nearly doubled, also, increasing from $51,000 to $100,000. This is due in part to an increase in the number of Civil Service positions and in part to promotions within the ranks and increases provided at the last session of Congress by the liberalized compensation act of 1928.

The personnel of the division has also undergone considerable change in the past few years. In 1924, 18 regular investigators were employed on a full-time basis; at present there are 33. The bureau, however, has always had active cooperation from investigators in universities and private institutions, many of whom have held brief appointments from time to time, particularly during the summer season, but who have in most cases continued their interest throughout the year. Among those recently employed on this basis are Dr. H. B. Bigelow, of Harvard University; Dr. R. E. Coker, of the University of North Carolina; Dr. M. M. Ellis, of the University of Missouri; Dr. Herbert Fox, University of Pennsylvania; Dr. C. H. Gilbert, of Stanford University; Dr. F. G. Hall, Duke University; Prof. Chancey Juday, Dr. George Kemmerer, and Dr. Samuel Lepkovsky, of the University of Wisconsin; Dr. G. C. Price and Dr. J. O. Snyder, of Stanford University.

In reviewing the changes in personnel during the past two years I must first report the passing of the bureau's most beloved and respected counsellor, Dr. C. H. Gilbert, who died at Stanford University on April 20, 1928. Others have been separated from the service but can not be counted as losses because they are still with us in fisheries work, performing greater services than when official members of our scientific staff. These are Dr. and Mrs. C. J. Fish and M. C. James. Dr. Walter Koelz and I. L. Towers have resigned from the field staff, and from the office staff we have lost J. V. Greene and R. A. Coleman by retirement and W. A. Lyons by transfer. The scientific staff has grown by the appointments of Dr. A. E. Hopkins and Dr. R. H. Luce to the oyster work; Dr. Stillman Wright, B. H. Hill, and H. J. Deason have joined the Great Lakes staff; H. A. Hanson, V. E. Heffelfinger, E. W. Bailey, and Henry M. Bearse are engaged on the Atlantic coast investigations; Seymour Smith and A. C. Taft are working on salmon; E. W. Surber is surveying the upper Mississippi River; Miss Louella E. Cable is stationed at Beaufort, N. C.; and Alonzo Thomas, W. C. Neville, and Miss Helen Guy have been appointed to clerical and laboratory aide positions in the field and Clarence E. Werback in the Washington office.

Of interest to all of you is the prospect of increased compensation during the coming year. The liberalized compensation act of 1928 gave each of you a slight increase in salary on July 1, and there is considerable hope of further congressional action in raising the salaries of the lower paid employees throughout the Government service. Furthermore, reclassification of the field service is now under way. Much work in this direction has already been accomplished by the Personnel Classification Board, and although not everyone in the field service will receive salary increases on that account, all will be benefited by the adjustment of salary ranges to accord more closely with the duties actually performed. In 1925, through the first reclassification act, the salaries of the bureau's investigators were materially increased and placed on a basis comparable to those in first-class universities and further improvement in salary rates are very encouraging.

I have been recounting for you a very satisfying picture of the material success in bureau organization, but I believe there are prospects for great future development. At the last session of Congress Representative White, chairman of the Committee on Merchant Marine and Fisheries, introduced a bill providing for a well organized program of expansion for the Bureau of Fisheries during the next five years. This bill passed both House and Senate but received the pocket veto at the end of the session. The matter is not dead, however, for I understand that the same bill is to be introduced again with some possible modifications that will probably win for it united support. Scientific inquiry's part in this program of expansion includes increases in appropriations during the five years amounting to $250,000. These funds are to provide both permanent salaries and operating expenses for investigations in all of the major geographical districts. On the North Atlantic coast the haddock, flounder, and mackerel should receive adequate attention; in the Middle Atlantic section the shad and menhaden; in the south Atlantic and Gulf the mullet and shore fishes and shrimp; on the Pacific coast the salmon and the herring; and in the interior waters expansion of the work in experimental fish culture and mussel propagation and the study of stream pollution are projected.

As the commissioner has told you, our present fortunate position is due to the public's appreciation of your work in the applied science of fisheries biology. If this program of future development is to be accomplished it must be by continued devotion to the work of making fisheries conservation practical and effective.

REVIEW OF THE FISHERIES SITUATION

LEWIS RADCLIFFE, *Deputy Commissioner of Fisheries*

Few lands have been blessed by so rich fisheries resources as the United States. And yet, within a century after the Declaration of Independence, or on February 9, 1871, to be exact, a joint resolution of Congress was approved appointing a commissioner of fish and fisheries to conduct investigations to determine the facts and causes of the alleged diminution of the fish supply and the feasibility of remedial measures. At this early period an established market demand for certain species of fish and shellfish existed. Included in this list were bluefish, cod, halibut, trout, mackerel, mullet, shad, salmon, squeteague, clams, lobsters, oysters, and terrapin. Because of their abundance, popularity, or importance in the markets of the country they may be classed as staples in the fish trade of that period.

During the first phase, when we depended very largely on a relatively small number of well-known staples, the record catch was attained for such epicurean products as clams, lobsters, oysters, shad, sturgeon, smelt, and whitefish. Following are the maximum catches, compared with the present level of production:

	Maximum in pounds	Present level in pounds
Clams	23,000,000	13,000,000 (Atlantic).
Lobsters	30,000,000	10,000,000
Oysters	186,500,000	115,000,000
Shad	50,500,000	12,000,000 (Atlantic).
Sturgeon	18,000,000	800,000
Smelt	1,800,000	700,000 (Atlantic).
Whitefish	21,500,000	4,500,000
Cisco (Lake Erie)	39,000,000	2,350,000
Total	370,300,000	158,350,000

Other illustrations, such as the Atlantic salmon, will occur to you. Since the nineties other species have attained their maximum and are now on the decline, but prior to that time the dangers of depletion were less evident and the effects of replacement less noticeable. This is particularly true of regions such as the Pacific and Gulf States, more distant from the great centers of population in the East.

During the second phase, beginning about 1900, new fisheries were being rapidly developed to supply the demands of our growing population or to make up for the decline in the catch of the staples. In the New England

area the fishery for haddock has assumed a position of much greater importance, as shown by the following:

Year	Catch in pounds
1900	46,000,000
1910	62,500,000
1920	85,000,000
1925	108,300,000
1928	180,000,000

In the Chesapeake area, with the development of markets, the catch of the blue crab increased to one of large proportions, as shown below:

Year	Catch in pounds
1880	3,300,000
1890	9,500,000
1915	50,300,000
1925	30,000,000

In the South Atlantic and Gulf States the shrimp came into prominence, the catch of various years following:

Year	Catch in pounds
1890	8,200,000
1908	18,500,000
1918	46,000,000
1923	70,800,000
1927	98,800,000

In the Great Lakes the catch of the three species of pike-perch increased from 10,000,000 pounds in 1903 to 27,000,000 pounds in 1915 and declined to 12,000,000 pounds in 1927. In no fishing area has a fishery developed with greater rapidity on so large a scale as that of the pilchard fishery of California, the basis for the valuable sardine canning industry, the catch for certain years following:

Year	Pounds
1904	1,000,000
1916	15,000,000
1917	165,000,000
1918	157,000,000
1925	315,000,000

Economic factors limited the catch of 1920–1922.

With the development of markets for canned tuna, fisheries for the several species of tunas grew with great rapidity. Prior to 1915 the catch rarely exceeded a half million pounds. In 1915 it was 21,500,000 pounds; in 1920 nearly 46,000,000; and in 1925 nearly 55,000,000. The catch of Alaska herring increased from 9,400,000 pounds in 1915 to 28,800,000 pounds in 1918 largely as a result of the bureau's introduction of the Scotch cure; fell off during the postwar depression; then increased to the record catch of 77,000,000 pounds in 1925. By the granting of permission to use pilchards for reduction purposes the British Columbia fishery assumed large proportions almost immediately. In 1924 the catch was 2,750,000 pounds; in 1926, 97,000,000 pounds; in 1927, 137,000,000 pounds; and in 1928, 161,000,000 pounds.

Attention has been called to some of the more important instances in which the catch of a species has been greatly increased during the last quarter of a century. Many others will come to mind, such as the fisheries for whiting and tilefish off our northern coast; Spanish mackerel, groupers, and drums in the South; suckers and sheepshead in the Great Lakes; and barracuda, halibut, mackerel, and sablefish on the Pacific coast.

It is by the development of such fisheries that we have been able not only to maintain but to increase our annual yield. This period of exploitation is drawing to a close. Except possibly in the North Pacific, there remain no huge undeveloped fishery resources. For nearly every one of our great fisheries the maximum catch has probably been attained. The North Atlantic bank fisheries are being prosecuted with the greatest intensity in their history; the halibut fishery of the North Pacific is now prosecuted coastwise for a distance of about 1,800 miles; failing in a sufficient domestic supply off the coast of California, the volume of fishery products taken from the waters off the coast of Mexico

has risen with amazing rapidity. As a result of the greater demand created through improved methods of merchandising millions of dollars are being invested this year in new vessels and gear preparatory to taking a still greater toll of our fish supply. This is the situation which confronts fisheries biologists, executives, and the fishermen. It is a situation which commands the best thought and effort we can give to it.

The two principal phases of modern fisheries biology, as we know it to-day, are (1) the conservation of our national fishery resources, and (2) supplementing the natural supply through the development of aquiculture. In this respect we are far behind agriculture, in that long ago the natural supply of land products ceased to be of primary importance except in such fields as forestry. In the fisheries we still place our chief dependence on the natural supply and must continue so to do until the science of aquiculture is much further advanced than now.

Your first concern is to keep posted on the status of the fish population of each important species, to detect any changes, and recommend measures needed to maintain the fishery at the highest practicable state of productiveness. In Alaska you not only do this, but you have an opportunity to make practical application of your findings. The biologists of the bureau are, therefore, on trial with respect to the perpetuation of such anadromous fishes as the highly priced salmons, which furnish nearly half of the world's supply.

The biologists of the halibut commission are also on trial as to their ability to solve the problems of a great sea fishery. Unless they can succeed, what of the future of our other great sea fisheries, shall they be swallowed up by the greed of man? We have the further complicating factor of the ability of nations to put into effect the recommendations of the fisheries experts regarding fisheries of the high seas. Heretofore such attempts have not been particularly successful. As to their failure I am inclined to believe the biologists share the burden because their recommendations were not always convincing to the layman and not well founded on a sound investigative program. We have a comparable problem on the Great Lakes; but here I expect final success, because if the States themselves do not do the things they should do, we shall see a centralization of authority in an international fisheries commission or some other central organization, which will supersede the present division of authority. Many problems of the high seas, such as the conservation of whales and other marine mammals, the great sea fisheries of the North Sea, and our North Atlantic bank fisheries await settlement.

The annual catch of cod alone on the east coast of North America exceeds a billion pounds annually, of which 49 per cent is credited to Newfoundland, 20 per cent to Canada, 17 per cent to France, 12 per cent to the United States, and 2 per cent to Portugal. With the development of factory ships for freezing, canning, and reduction of fish waste the intensity of fishing and the number of countries and vessels taking the toll of our Grand Banks fish supply may be expected to increase. We have already observed what is happening in the haddock fishery. Will it not be necessary to restrict fishing thereon by international agreement? Will the biologists be prepared to state what restrictions are necessary when that day comes? In 1927 over 25,000 whales were killed. The seas of the earth are combed for these huge mammals. Great factory ships with reduction plants, storage capacity of upwards of 3,000,000 gallons of oil, and some with provision for drawing the whales on board for cutting up, with a fleet of smaller killer vessels visit the Antarctic each year. Already the League of Nations is giving the matter of conserving the supply of whales attention, but can the biologists make definite recommendations as to what steps should be taken?

The Passamaquoddy power project excellently illustrates what I have in mind. This hydroelectric power development will control the tidal flow between two natural basins in this region, which happens to be the center of the young herring fishery on which the sardine-canning industry depends. Suddenly and without previous warning the biologists are called into conference to determine the probable effect of this development on the fishery. Our knowledge of the oceanography of the region and of the factors that make this huge mixing bowl so suitable for the herring is very incomplete. Without this mixing this body of water would be very little different from other points on the coast. Unless the advice given as to the probable effect of this development on the fisheries is correct, it will tend to discredit fishery biologists. This is an excellent illustration of the need for accumulating the necessary knowledge about our great fisheries, for from now on we may expect similar calls at any time.

In our inland-water areas we find the fisheries situation greatly complicated by the results of our rapid industrialization. Not only are the fishes more susceptible of attack, but we have added to the hazards of the fishes themselves through deforestation, pollution of our waterways with domestic sewage and trade wastes, destruction of the nursery grounds by reclamation and drainage projects, cutting off the runs of anadromous fishes en route to the spawning grounds by dams and other barriers, and the rapid development of good roads, the increased use of the automobile, and the shorter hours of labor making the inaccessible places accessible to the growing millions of our people seeking healthful recreation in the great out-of-doors.

In all fishing areas the strain on the supply has been greatly increased by the increased efficiency of fishing gear, boats, and other transporting agencies; by improvements in methods of merchandising and distribution; by the better understanding of the richness of aquatic foods in elements essential to good health. To illustrate: The first otter trawler for engaging in our North Atlantic bank fishery was built in 1905. In 1929 it is expected there will be at least 50 such vessels operating out of New England ports. The use of the motor truck for transporting the catch to market is of comparatively recent origin, yet in 1924 New York City alone received 22,000,000 pounds of fishery products by this route; in 1928 it is estimated that the volume of products shipped in this manner will be double the 1924 figure, or about 45,000,000 pounds. The packaging of fresh and frozen fish as fillets, steaks, sticks, and pan-dressed, initiated in 1921, has grown in seven years to a great industry utilizing about 150,000,000 pounds of fresh fish. The landings of haddock, the principal species packaged, increased from 46,000,000 pounds in 1900 to 62,500,000 pounds in 1910, to 85,500,000 pounds in 1920, to 144,000,000 pounds in 1927, and 180,000,000 pounds in 1928. On the South Channel grounds, along Cape Cod, the catch per acre per annum increased from 16 pounds in 1910 to 50 pounds in 1925 and 114 pounds in 1927.

How long may we expect the fishery to stand up under such intensive fishing? Thus, on every hand the strain on the natural supply is increasing at a rapid rate, and when the strain is reflected in greatly reduced catches you biologists will be harassed to supply a sure and instant relief. At least one progressive fishing company has already considered adding a biologist to its staff. This is but one of many signs of a growing interest in determining the present status of our fisheries and insuring a greater degree of control over fishing operations. Eight years ago when we were rapidly forging ahead in the field of fisheries technology, we recognized that ultimately this would bring about the situation with which we are faced to-day, but few of us realized the rapidity with which the change would come. Improvements have resulted in greatly enhancing the value of fish. Enhanced value and heavier investments in turn result in raising the importance of the biologist and his output.

Confronted with this possibility of the exhaustion of the natural stock, our attention is turned to discovering means for replacement or additions to natural stock. With the drawing to a close of the era when replacements may readily be made, we are brought to the second phase of fisheries biology to which I referred, namely, aquiculture. What is more natural than for man to attempt to duplicate his successes with land crops by attempting to grow water crops? Fish culture as we know it in this country, particularly as practiced by Federal and State Governments, is, of course, a phase of aquiculture. Just as in many other branches of human endeavor, the development of the mechanics of operation has taken precedence over close study as to means of improving the product itself. In fish culture we have been more concerned about volume of output and not enough concerned with what happened to the billions of nearly helpless fry released to shift for themselves. Fortunately in recent years we have been rapidly developing the fundamentals which will assure a larger return on our investment and do not place as much trust in numbers alone.

Among the bureau's activities are numbered various phases of experimental work in aquiculture. At Holden, Vt., we are solving the problems of trout-cultural operations—feeds, diseases, selective breeding, etc. At Fairport, Iowa, the growing of basses and other warm-water fishes—fertilization of ponds, introduction of forage fishes, and other experiments to increase the productivity of water areas.

In the coastal waters remarkable success is being achieved in the development of oyster farming. To-day the greatest retardants to the rapid increase in oyster acreage under cultivation are antiquated State laws, which virtually

prohibit engaging in the enterprise on a scale necessary to attract capital. We now produce about 17,000,000 bushels of market oysters annually. We have areas of suitable bottoms for cultivation capable of producing 100,000,000 bushels, or six times the present harvest.

Terrapin culture and mussel culture experiments now in progress belong to the field of aquiculture. In addition to what the bureau is doing, other examples will readily come to mind, such as goldfish farming, commercial trout culture, and the raising of bait minnows.

Thus, as our natural stock of fishes decreases we will place greater dependence on aquiculture for replacement. But how much food will an acre of water grow? How does it compare with the land in productivity? While our knowledge of the subject is far from complete, glimpses here and there indicate that water areas can be made fully as productive as land areas and in some cases will far exceed the land in productiveness. A few random illustrations may be of interest:

	Pounds per acre
Oyster meats, Atlantic coast	100
Fish, South Channel, Cape Cod (catch)	122
Beef, cultivated land	140
Fish, Lake Pepin, Mississippi River (catch)	145
Carp, cultivated ponds	150
Bluegill sunfish, Fairport Pond	272
Pork, cultivated land	300
Goldfish, Fairport Pond	450
Clams, coast of Washington (catch)	535

A careful analysis of the quantity of water crops that may be produced per acre, their market value, and the cost of operations I believe will convince you that the day is at hand when aquiculture will be an established and attractive pursuit capable of adding its quota to our harvest of the natural stock and replacing shortages or caring for growing demand for water products.

The subject of aquiculture has been dealt with at considerable length because of the increasing attention which the subject is receiving and fully merits. We shall continue to place dependence on the great sea fisheries for the great bulk of the catch, and these merit serious study for their proper administration. As biologists observant of the slowness with which your recommendations are acted upon, you undoubtedly wonder at times whether your efforts are worth while, whether they are appreciated, and whether you should perhaps have chosen some other calling. As to their being worth while, I know of no period in the history of this bureau when it has had a more practical program or when it has rendered greater service. This is reflected in increased appropriations, more demands for service, and greater reliance on the recommendations which the bureau makes. Our problem is to keep pace with the demand for information necessary to the proper husbandry of our resources.

Usually when the authorities awaken to the need for action they want to know immediately what to do. We have had an illustration of that in the Great Lakes States. Some morning we will find the haddock question on our doorstep and will be expected to state the remedy at once. Thus, the demands for our service will increase, and if we give good service our work will be appreciated. Not only that, but just as happened in the technological section, our trained experts particularly in such fields as aquiculture, will be in demand by industry itself. I know of no more fitting demonstration as to the practicality of our program of research.

WHAT BIOLOGY MAY DO FOR THE COMMERCIAL FISHERIES

H. F. Taylor, *Vice President for Research, Atlantic Coast Fisheries Co.*

It is scarcely necessary to expatiate on the desirability of some mutual understanding between the marine biologists and those who are engaged in the fisheries industry. The fact that the bureau has honored me with an invitation to discuss this subject sufficiently demonstrates that the need for mutual understanding is recognized and that the bureau proposes to do what it can to promote it.

Recent trends and developments in the fish industry have been such that they raise sharply certain biological questions; to answer these questions it

may be necessary for the marine biologists to place emphasis in directions other than those they have been following. Perhaps it may be advisable first to describe the trends in the fish industry, the biological significance of which will then become apparent.

Until a few years ago, of the four great basic industries, agriculture, mining, forestry, and fisheries, only the fisheries remained unaffected by what is variously known as the "industrial revolution" or the "scientific revolution." The industry was (and still is, to a large extent) in the hands of individual fishermen, small firms, partnerships, commission merchants, and retail stores. Any man with a few dollars capital could buy a boat and go fishing or set up in business as a commission merchant or a retailer. Needless to say, in this kind of organization of the industry no specialization was possible, and nothing much was to be hoped for by way of applying scientific methods to the capture, preservation, and distribution of sea foods. Losses in this kind of industry are necessarily great and quality far too often poor.

The corporation has now made its appearance in the fish industry, with its specialists in scientific research, engineering, business analysis and accounting, marketing, advertising and traffic problems, factory management, and finance. Everybody now knows the efficiency of a diversified and specialized corporation as compared with the one-man business. Though for sentimental reasons we may not like corporations, the trend of modern business has been in their direction for many years, until they now dominate it in almost every branch. In the fish business the research laboratory is applying chemistry and physics to the solution of the problems of preservation, preparation, and distribution of perishable sea foods; engineers are designing and installing intricate machinery for manufacture and in capture; ocean areas are being charted into units; elaborately equipped ships fish these areas and are daily in touch by radio with headquarters, from which operations are directed; experts in merchandising are applying the arts of salesmanship to the nation-wide advertisement and distribution of the products. We are just now adding a biologist to the Atlantic Coast Fisheries' research department, whose function will be to apply biology in operations at sea.

All of this is not intended to imply that the corporation will or can take over the fish business. Some 300 species of fishes are commercially exploited in this country, each having its locality and period of abundance, and most of them are classified into size groups. They do not all readily lend themselves to nation-wide distribution. Much of this business must for a long time be in the hands of local firms, if for no other reason than because there are too many varieties of fishes for the general public ever to become familiar with all of them and their relative merits.

Highly important in a national system is standardization of process and a product which can be nationally advertised. Plants must be kept going as near continuously as possible and prices must be stabilized. These requirements point to species of fishes that occur in large numbers and can withstand heavy exploitation. Fisheries biologists know that there are relatively few such species—cod, haddock, salmon, mackerel, shrimp, oysters, and halibut. These species differ among themselves, as we know in a general way, in their fecundity, rate of growth, age at maturity, and general ability to support extensive exploitation. Greater exactness in this knowledge is highly desirable. Mistakes of judgment of fisheries companies based on assumptions or inadequate information regarding supplies of raw materials of these and perhaps a few other specis would be disastrous, not only to the erring company but to the fishery as well. My first suggestion to the biologists may therefore be formulated as follows: Studies of the biology of these important species of fishes should be made with a view of determining the extent to which they may be exploited safely. Incidentally such studies can not fail to yield other valuable information concerning locations where they may be taken most economically, periodic fluctuations of abundance, and possible measures of promoting the supply.

Viewed in a broad way, the fisheries industry may have a very prosperous future ahead of it, or it may have to be satisfied with a much more modest success. Populations of the world are rapidly on the increase. A glance at history shows that the older countries have been living to a large extent off the new frontier countries, but these latter are rapidly outgrowing frontier conditions and are becoming industrialized and thickly populated on their own account. Argentina, Australia, New Zealand, and Canada may not for many more years

continue to ship meat to the older countries. The sea covers 71 per cent of the earth's surface, receives 71 per cent of its quota of sunshine, and is the world's great reservoir of chemical compounds necessary for life. The ocean basin hoards untold millions of tons of phosphates, nitrates, and other chemical building materials, including the rarer ones that are being found essential to life. Every factor of material and energy is present in the ocean in magnitude almost beyond human conception. Can the fishing industry look upon the sea as a vast future source of food for mankind, or must we, at great effort and expense, solve the problems of capture, preparation and distribution, get things going, establish distribution, create public demands, and then in two or three years see international commissions hoisting danger-signals of depletion and bills introduced in Congress and legislatures to restrict capture here, close seasons there, and impose limitations elsewhere? The total of living substance extracted from the sea is a merest trifle compared with the total productivity and a tiny fraction of the foods otherwise available to man. Is it illusory to suppose that we can really exploit the sea, and is the cycle of life so ingrown that we can really get only a trifling amount from it?

Coming more specifically to the haddock: We have lately been hearing much alarmed discussion of a possible depletion of this valuable fish. Figures when printed, containing long strings of zeros all divided up into groups of three, are impressive, especially since they are rapidly growing larger. In pounds we are taking, say, 250,000,000 in 1928 and may take, at the present rate, nearly 300,000,000 in 1929.

I have with me the operating charts for the coming year for our own company, which, by the way, is not public information, but I do not mind giving you a few facts from it. The anticipated production from our vessels, of which there will be 18 this year, is 56,697,000 pounds of haddock to be landed in New York and Groton, Conn., which will produce 20,111,000 pounds of finished fish for the market, of which some goes to every State in the United States. Every State in the Union is covered, and a great deal of it goes to California, I might say, and to the North Pacific States, from the Atlantic coast.

Until December, 1927, the Atlantic Coast Fisheries Co. had 4 trawlers in operation. From December, 1927, to June, 1928, 9 trawlers; in the summer of 1928, 7 trawlers with 2 laid up; from October 1, 1928, 14 trawlers; and within the next few weeks 18, which will probably operate the year round. This represents a new addition of 14 trawlers by our own company within one year. The Boston fleet has also been increased by some new trawlers, and several vessels of other types have been fitted for the haddock fishery.

This rapid expansion of the haddock fleet raises some important biological questions, chief among which is, of course, danger of depletion. Now, if it is important for anybody to know the extent to which haddock can be captured with safety, it is the fishing companies, which must do much work and invest large amounts of capital on the prospects. We have, therefore, every reason to desire the truth without exaggeration or disparagement. I have seen no valid evidence, other than mere magnitude of figures, that indicates one way or another whether or not the haddock is in danger of depletion; but we do know that depletion has occurred in the North Sea, which definitely proves that there is a real and attainable limit in the extent to which we can take this fish. Depletion is indicated when the average size of individual fish diminishes. This sign does not occur suddenly, but gradually. We have had, during the past November and December, a marked drop in catch of haddock on the banks, and there has been some concern lest this means a beginning of depletion. That it probably does not is indicated, first, by our records, which show that this drop in catch of haddock occurs regularly every year during November and December, sometimes in January, and is followed from January onward by a marked increase; second, by the fact that the short catches are not marked, so far as we have observed, by any diminution in average size of individual fish; and third, that the drop in catch occurred suddenly, which is contrary to experience in depletion. An important service to the industry can be performed by a careful watch for signs of depletion, those mentioned above and any others that the biologists can discover.

If this November-December scarcity of haddock does not represent depletion, but is temporary, what are its causes? Even if it means nothing worse, it imposes an extra cost on capture and disturbs the market. It would be important to discover what becomes of haddock during these months. Probably the cause will be found in a horizontal or vertical migration of the haddock from their usual habitat to others for one or more of the following reasons, viz, (1) scarcity

of food, (2) presence of enemies, (3) some connection with spawning, (4) temperature or salinity of the water, or both. Of these four possibilities, scarcity of food or conditions of the water appear most likely, because dogfish are out of season in these months, and spawning does not begin with the haddock until January or February. We have no information on the food supply of the haddock during these months. Since, however, the food of the haddock is bottom invertebrates, whose food in turn is the "rain" of organic matter from the surface, we would expect most rapid growth of the haddock in the months following vernal bursts of planktonic growth at the surface, allowing for a certain lag between the two. It may be that by November the season's growth of bottom invertebrates is almost exhausted; but even if this theory is correct, it tells us nothing of the whereabouts of the haddock. It would be useful to know this. Records show that the peak of capture of haddock on Georges Bank is in March, April, and May. When they are scarce on the bank in November, they are more plentiful in shore waters. The following figures, taken from our records, show the average catch of ground fish per boat per month for certain points, in thousands of pounds (000 omitted):

Year	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
1925	166	252	364	276	382	348	266	307	358	246	259	218
1926	180	265	386	295	431	424	375	473	440	340	343	361
1927	259	382	488	356	451	500	408	508	495	358	306	258
1928	366	462	543	504	606	400	447	424	354	403	262	------
Average	248	340	445	358	467	418	374	428	412	337	292	279
Percentage 1928 over or under 1927	+50	+21	+12	+42	+34	−20	+9.5	−17	−28	+12	−14	------

These figures (which are not corrected for the influence of stormy weather on fishing) agree with a theory that haddock seek the bank for spawning, then, after feeding there through the summer, scatter over the Gulf of Maine in fall and in shallower shore waters in winter. They do not contain any evidence of depletion. From all the above it is obvious that a general knowledge of the movements and habits of haddock would be useful.

So far only the questions of depletion and facilitation of capture of a steady supply of haddock have been considered. If we wish to exercise foresight while we have haddock, can we do anything whatever to promote its productivity? For one thing, I have suggested that during the spawning season it would not be difficult to fertilize mature eggs aboard the trawlers and dump them overboard immediately. In order to determine the feasibility of this procedure some little investigation would be necessary to ascertain (1) whether the eggs and spermatozoa of trawl haddock are still viable when they come on deck and (2) whether a sufficient number would be mature where fishing is done to justify the effort. Perhaps the bureau is already prepared to answer these questions; but if not, the necessary investigation might profitably be done at an early date. I can speak for the Atlantic Coast Fisheries Co., that we are willing to cooperate with the bureau in every way looking toward the carrying out of such a project.

This proposal, however, only raises another question. To what extent is the depletion of the haddock fishery (or of any fishery) caused by deficiency of fertilized eggs? If the limiting factor that determines the amount of haddock is the number of eggs hatched, then the obvious thing to do is to hatch every possible egg. I have believed for years that the matter was much more complicated than this simple proposition would indicate. The prodigious fecundity of haddock and, in fact, of most fishes is well known.

Let us estimate an average production of 300,000 eggs by each female haddock and accept the estimate of Bigelow and Welsh of a haddock population of 150,000,000 of marketable size in the Gulf of Maine. These will, of course, not all be of the same age class, but more likely in some such ratio as four of the age of 5 years, three of 6 years, two of 7 years, and one of 8 years. If this assumption be correct, 40 per cent of the adult haddock population, or 60,000,000 fish, must reach maturity each year. If 100 per cent of eggs survived, 200 females would be sufficient to supply this number.

Now, let us further suppose that we catch annually 250,000,000 pounds of haddock, averaging 3½ pounds per fish, or 71,500,000 individual fish, leaving

in the water 78,500,000, of which 50 per cent, or 39,250,000, are females. We thus have 39,250,000 mature females to produce enough eggs to make up for the failure to survive of the eggs of 200 females, which would perpetuate the supply if all survived. This gives us a survival ratio of one in 197,500, or 0.00051 per cent. If we fertilize all the eggs from all fish caught, we change the necessary survival ratio from 0.00051 per cent to 0.000267 per cent, against which must be applied some unknown factor that represents the diminishing chances of survival with increasing numbers of eggs.

Now, it will be noticed that in the above reckoning we are catching 71,500,000 haddock, while only 60,000,000 are reaching maturity, and an unknown number are being destroyed by enemies other than man. If the estimates be anywhere near correct, we are already depleting the fishery; but we must take it for granted that Bigelow and Welsh did not propose their estimate of 150,000,000 population as much more than a guess. Biologists might secure some factual data from the average catch per set of the trawl, the area of bottom covered, and the number of sets. These calculations could be further refined by a study of age-group ratios and of the average fecundity of mature haddock.

Of course, the above figures are merely assumptions. The point is, however, that where conditions are favorable for propagation a relatively small number of fertilized eggs is sufficient; but if conditions are unfavorable no number of eggs will avail to perpetuate the kind. Conditions on the Pacific coast being favorable, the shad have increased in number without artificial propagation.

The purpose of these remarks is not at all to criticize the bureau's activities but to give point to the suggestion that what is needed is a general theory of population of marine animals. The fixed limit for any one species would seem to be, in most cases, the amount of food available for that one species; or, perhaps, the basic characteristic rate of growth. If we remove from the water more pounds of haddock than the available haddock food is able to nourish, then we are headed inevitably toward depletion. What would happen under such circumstances would seem to be what does happen in depletion; that is, the fish are smaller and smaller when caught, or, in other words, they do not grow to a sufficient size by the time they are caught. This again, however, is purely theory, though it may be part of the truth.

To illustrate, suppose the haddock had no enemies at all, not even fishermen. The bottom would be scoured for food, and the total growth would be determined by the amount of food; but growth would be slow in the extreme. Other species competing for this food would further retard growth, and vigorous body activity necessitated by the unceasing search for food would still further retard it.

Now enter the fishermen. By reducing the number of fish the fisherman reduces competition, and likewise the necessary activity of the haddock in search of food, actually increasing the amount of fish up to a certain point, this point being reached when the number of fish is not sufficient to consume the available food, and the rate of growth is that of fish which get all they can eat, it being assumed that the net used filters out only the larger individuals. This is, of course, not a complete theory, for it becomes more complicated the more we contemplate the problem. It may boil down in final analysis to how fast a fish can grow—the characteristic or basic rate of growth of a fish under the best of conditions. If, on careful analysis, such a theory should be confirmed, it would be important experimental work to measure the basic rate of growth.

Our European friends have proposed the theory that because young plaice are more rapid growers than old ones, it is fundamentally sound to catch them to the limit. Survival may, and undoubtedly does, vary greatly from year to year. Pelagic haddock eggs are part of the plankton during their natural period of incubation and, as such, are subject to being consumed by organisms that feed on miscellaneous plankton. The time of incubation of haddock eggs is a function of temperature; that is, temperature near the surface. Thus, in seasons of low surface temperatures the eggs would be subject to being eaten for a longer time than in seasons of warm surface temperature, when they hatch quickly. Coexistence of larvæ with an abundance of copepods on which they feed may have much to do with survival. Other questions relating to the life history of the haddock require study. Harold Thompson in Scotland states that in "good" seasons as much as 50 per cent of haddock may spawn at the end of the second year and all in the third. Bigelow and Welsh say that the haddock spawns from the first year on, and others say that the haddock

spawns from the second year on. This certainly does not agree with our information regarding American haddock. It has an obvious bearing of importance on the question of the productivity of haddock.

Again looking to an improvement in haddock productivity, we have the question of enemies. Technology must solve the dogfish problem. Of course, no ocean fish can ever be exterminated by exploitation—haddock or dogfish. Depletion merely goes to a point beyond which capture is unprofitable, and stands at an equilibrium thereabouts. Biologists might be able to tell us whether the dogfish problem is really as serious as most fishermen think. Assuming that all dogfish caught at present are killed before they go back in the water, no advantage to the fishery itself would be brought about by utilization unless a special fishery for dogs were created. In fact, the advantage might be the other way about, for dogs thrown overboard perhaps sink and become food for the bottom invertebrates, indirectly feeding the haddock. The same argument may well apply to the disposal of guts at sea, concerning which there has been some thought of utilization. Perhaps we might work out some means of utilizing them, but would it be worth while? Pollock do not seem to be sufficiently abundant, at least on George's bank, to be a serious factor as an enemy of the haddock; our figures show less than 1 per cent of the total catch to be pollock. In other parts of the gulf they may be more serious.

Another factor that may have a bearing on the future of haddock is the practice of bringing in scrod. It is interesting to note that in the British haddock fishery the total catch on all grounds diminished from 316,000,000 pounds in 1906 to 174,000,000 pounds in 1913, and that meanwhile the percentage of the classification "extra small" (9 inches long) increased 64 per cent from 1905 to 1911. Because of the practice, at that time on the increase, of bringing in more and more small fish, it can not be determined what part of this increase in small fish was caused by the growing practice of bringing in the small ones and what part by depletion. Perhaps the former was a contributing cause of the latter, though the theory of ocean population I have hinted at would say not.

It is also interesting to observe that the height of the British capture was about what ours promises to be this year; but that does not include the part taken by continental countries. If our waters are comparable in productivity with theirs, we have at least some rough idea of the order of magnitude of possible haddock fishery, which should be a great deal larger than it is at the present time.

Tagging experiments as far as we have had opportunity to know, seem to show that haddock move about considerably in and around the Gulf of Maine. If this suggestion is confirmed it may prove that intensity of fishing in South Channel is not likely to deplete that particular area because it is supplied by migrations from the entire gulf. We shall await with much interest further reports on this work.

So much for the haddock; it is discussed at some length because just now it is of widespread interest. Similar discussion might be made of other species, such as mackerel. The bureau is already studying this fish, and the results that flow from this study will be timely indeed, for without a doubt the mackerel will be exploited to its limit before long.

I wish to say in conclusion that the work already being done by the bureau is of the greatest value, and its officers may be sure that the results are being followed closely and profitably. The monumental works of Doctor Bigelow and his associates deserve especial praise, and large as they are, it is to be hoped that they are only a beginning.

SUMMARY

1. Studies of the biology of the more important species of fishes should be made with a view of determining the extent to which they may be exploited safely.

2. Though specific data would, of course, be out of the question, it would be useful to derive from biological study some estimate of the extent to which the vast oceans can really contribute to the food supply and discover what factors, in a broad way, fix the limit to this supply.

3. A systematic vigilance should be maintained for definitely recognizable signs of depletion in the more important species.

4. With respect to the haddock, (*a*) Are we approaching the limit of safe exploitation of haddock? (*b*) What are the general movements of haddock in water that explain fluctuations in the supply at the usual points of capture? (*c*) Is it advisable to commence hatching operations for haddock aboard the trawlers? (*d*) Can anything be done about the enemies of the haddock? (*e*) How serious a matter is the practice of bringing in the scrod haddock?

5. A general theory of fish population of the sea is needed, comprehending the several factors which account for the number of any one species in ecological equilibrium at any one time, and including the effect of artificial hatching upon it.

PROGRESS OF FISHERY BIOLOGY ON THE ATLANTIC COAST

O. E. Sette, *In charge, division of fishery industries*

Judging from the program of this conference we are to hear about the details of fisheries investigations from the biologists themselvss, so that this will be only a general review of our progress, a review which I hope will help us all to get a perspective of the work that is under way and the relation of our specific problems to the ultimate goal of the bureau.

In making this survey of progress, I hope I will be forgiven if I reiterate some of the principles that govern the relative usefulness of our results. These, no doubt, are familiar to all of us, but it is dangerously easy to apply our thoughts and energies so closely to the immediate details that we may be sidetracked from the real aim of our work.

The previous addresses have made it clear that our prime duty, as concerns the marine fisheries, is to know at all times whether they are suffering from overfishing or other harmful influences, and to be able to tell administrators what may be done to correct such conditions. We will all agree that this is a large order. It involves a multitude of interrelated factors, many of them unknown or so poorly known that it is difficult to appraise their importance.

Stating the problem in simplest possible terms, we need to know, in the case of each kind of fish, whether they are growing scarcer or more plentiful, how much so, and what can be done about it, if anything. Although it is a simple matter to detect scarcity when it has become extreme, it is a more difficult thing to recognize it in its early stages when the remedy may be more readily applied without being so drastic as to find insurmountable opposition. For recognition of depletion in its early stages and of other moderate changes of abundance we require a quantitative measure. This involves the interpretation of suitable statistics by a biologist who knows, and knows intimately, the fishery with which he is concerned.

In many cases, or I might correctly say in most cases, no body of suitable statistics exists. This is due in part to the administrative and technical difficulties in the way of collecting the right kind of statistical data, but it is due also to the failure of the biologist to study his fishery and design a method suitable to show its changes in abundance. There are some general principles in the collection of statistics which will show real abundance, such as fixing the relation between a quantity of catch and a unit of fishing effort, but the definition of practical units is a thing that varies in each fishery and must be worked out for specific fisheries and specific localities.

At one time I was under the impression that it might be possible to have a statistical program that would automatically record the data on the fisheries so that they would be available when wanted by the biologist at a future date. Perhaps it is possible to do so, but our experience in the division of fishery industries is that it is exceedingly difficult to mobilize the funds and personnel for such work unless it can be shown that these statistics will have early application to the problems of the fisheries. Even where statistical programs are under way, they will fall into disrepair unless put to use through biological analysis. After all, the statistics are but the biologists' instrument for measuring abundance, and we all know that it is difficult to get an instrument unless the powers that be are convinced that it will be put to good use. Furthermore, we know that an instrument, neglected and allowed to go unused, will almost certainly be found in disrepair or be lost entirely when it is finally wanted.

This is but an analogy, and I have always been suspicious of analogies. I have used it because developments in fishery statistics have indicated the correctness of this illustration. The California system was planned and instituted by biologists, and if the biologist had not been putting those statistics to use the system, I little doubt, would have disintegrated by this time. In the Great Lakes region there is being developed a system with merit, but it would not have been done without the active participation by our fishery biologists and will be in danger of disintegration if not used by them during the next few years.

I may have dwelt overmuch on fisheries statistics, but it is perhaps not amiss, for we biologists are so likely to become immersed in our life-history studies and the like that we forget all about the prime question—the abundance of fish. We can, perhaps, leave the subject with the conclusion that the biologist can not expect to have his statistics furnished him ready-made but must bestir himself to see that this tool is being developed along with his other instruments. The statistical and biological studies must go hand in hand.

Given a quantitative measure of change in abundance, we may turn to the next question: What caused the change? We are interested in two kinds of causes, the so-called "natural" and the so-called "artificial" or man-made causes. Both may be traced to the three variables, which determine sizes of populations—birth rate, death rate, and migrations. In the fisheries these can not be directly observed. Unlike similar variables in the human population, we can not count the fishes in the sea, nor are their births and deaths registered. Instead, we must measure these factors indirectly. The progress of biology in the fisheries is contingent on the development of a technique that will enable us to observe these changes. The greatest advance in this respect was the scale method of determining ages of fish. By this means the fish in an adult population can be identified with their year of birth or "year-class," and the survival of a year-class may be followed from year to year. Thus, the study of age is not an end in itself. It is merely the means to an end, and unless employed for this purpose its value is largely lost.

Migrations may be studied both directly by tagging and indirectly by biometrical methods to determine subspecific differences. The data on migrations are invaluable in defining the units of fish populations, as well as to indicate whether or not conservation may be a local problem.

Once knowing the variations of birth and death rates, we are still far from our goal, for we must then seek their causes if we are to distinguish the natural ups and downs of the fisheries from the scarcity caused by overfishing. The very nature of the fluctuations and year-class changes from year to year may give a clue as to the cause, but a final answer must rest on a more intimate understanding of how the organism fits its environment and how it responds to changes in environment. In other words, we must look to a combination of oceanography, ecology, and physiology to lead us to the primary causes of fluctuations. Thus, we are involved in research that goes all the way from the economics of the fishery to the life history of the fish and to the responses of the fish to the physics, chemistry, and biology of its environment.

Obviously, to pursue all investigations to such fundamental causes is a well-nigh hopeless task. At least there would be danger of the fisheries being entirely depleted and experiments being stopped through lack of material before we arrived at the end of our quest for fundamental causes. Needless to say, the fishery biologist can bring aid to the forces of conservation long before he has reached the ultimate cause of changes in abundance. It takes careful planning to provide practical results at an early date, and for this reason the principles of investigation are being strongly emphasized.

Our progress in providing such contributions in the Atlantic coast investigations will probably indicate the extent to which this work has been directed to the more immediate requirements of the conservationist as well as to the laying of a suitable foundation upon which to build in the study of population flucutations.

Virtually all sciences begin with descriptive or qualitative studies. In the case of the fisheries this is the matter of describing and cataloguing our aquatic animals so that they may be recognized and their relationships understood. This phase of fisheries science was at the height of activity during the end of the last century, and by the early 1900's practically all important fishes

were known at least in the mature stages of life. That the work is not yet complete is shown by the discovery during the past year that what we thought was one species of sea trout on the Gulf coast is really two separate species, both of them of commercial importance.

With respect to the egg and larval stages of our fishes the descriptive work is far less complete. North Atlantic species are fairly well known, thanks to the oceanographic work in the Gulf of Maine and at the Woods Hole station. Middle and South Atlantic and Gulf of Mexico forms are very imperfectly known. The larval stage of the fish may be the most important in the natural fluctuations of the fisheries. It shows the most promise of providing the key to the causes for the ups and downs that are so prominent in many of our marine species and make it so difficult to distinguish the natural fluctuations from the changes in abundance due to man's activities. Progress in this branch may be reported by the bureau's Beaufort station, where weekly plankton collections are made for the purpose of catching fish eggs and fry, and where their study is being actively pursued. Since the eggs and fry of very few of the species are recognized, progress is necessarily slow; but each identified species narrows down the possible identity of the remaining kinds. Several of the species have already been identified, and results will be accelerated with the continuation of this work. Additional materials are being collected incidental to the mackerel investigations off the coast of New England and the Middle Atlantic States and will facilitate completion of this phase of the descriptive work. Such descriptive studies offer but little immediate aid to the administration of the fisheries except to explode some fallacies about the time and place of spawning and the possibility of man's interference with spawning grounds. There need be no apologies for this work, however, for it is the groundwork upon which our bricks may be laid in the future.

Let us turn to the progress of fishery biology on the Atlantic coast as distinct from the descriptive phase. The work in the northern portion has been favored by our long-established biological station at Woods Hole and the oceanographic studies in this region. It may not be exaggerating to say that through the cooperation of the Museum of Comparative Zoology the physical oceanography and plankton of the Gulf of Maine are better known than in any other body of water in North America; and, thanks to the Canadian Fisheries Expedition of 1914 and the work of the St. Andrews station, the Gulf of St. Lawrence and the coast of Nova Scotia are also partially studied.

Of investigations on other fisheries proper, that on the cod has shown the most interesting results. Since migrations promised to be most important in this fishery, an intensive tagging program has been pursued for a number of years. In the beginning the work was restricted to inshore grounds through lack of a suitable vessel for offshore fishing. With the *Albatross II* the bureau has been able to extend these operations to offshore grounds, such as Georges and Brown's Banks. Results indicate that in some localities the cod population is resident the year round. This is true of the Mount Dessert region along the coast of Maine. In other localities seasonal migrations take place. The Nantucket Shoals codfish form an illustration of this type of population. Most of the cod on this ground spend their summers on the Shoals but go to the coast of New Jersey and even down as far as Chesapeake Bay in winter, returning to Nantucket Shoals again in the spring. Preliminary results on the offshore banks indicate that the codfish there do not make regular seasonal migrations, but the fish move about considerably more than those in the Mount Dessert region.

We can, perhaps, derive the most important conclusion from the tagging work in the fact that it has demonstrated well-marked differences in the habits of codfish of various regions, which means that any study of the fluctuations of this fishery must deal with separate populations. This is complicated by the interrelation of grounds through the drift of cod eggs and larvæ. Oceanographic studies of the region have shown ocean circulation of a type that leads one to believe that few codfish grounds are self-perpetuating. A particular study was made of Massachusetts Bay and has shown that virtually all of the eggs that are spawned in the bay are carried out by ocean currents. Some of them, perhaps, finally arrive at Nantucket Shoals, while others may reach banks to the east and north. This makes it clear that although the adult populations on any ground may be fairly stationary, its increment of young cod depends on the success of spawning on other grounds.

Much work still needs to be done before the intricate problems of this fishery are solved.

In the State of Maine some interesting studies on the smelts have been going on for a number of years. These have been directed largely to the determination of subspecific differences, and one of the most interesting and practical results indicates that some races of smelt may be much more useful for stocking purposes than others.

The mackerel investigations are of more recent origin than those just mentioned, but it has already been fairly well established that the violent fluctuations of this fishery are due to either great differences in birth rate or great differences in mortality of the very young mackerel. It seems that in most years very few mackerel survive to augment the stock of commercial sizes, but occasionally we have a very good survival year. When this happens the sea is full of mackerel for the next several years. Two important conclusions may be drawn from this fact. One is that the natural reproductive rate of the mackerel is far more important in establishing the level of abundance than the influence of man's fishing activities. It can not yet be said that man's fishing activities do not have an effect on the population of mackerel, but their effect, certainly in the past at least, has been far less than that of natural fluctuations. The other result is that the abundance of mackerel is determined a year or two before they become of commercial size. This permits a prediction of general abundance of mackerel a year or more in advance. Before such predictions can become very reliable it is necessary to have followed several year-classes through their life cycle; but that some progress has already been made is shown by the results of our tentative prediction of the mackerel catch in 1928.

This prediction was made last March before the season opened. We estimated that the season's catch would be less than 88 per cent as large as that of 1927, perhaps very much less. The season just closed two weeks ago and the preliminary totals indicate that this season's catch was 79 per cent as large as that of 1927.

Biological work is badly needed in two other very important fisheries of New England. The intensity of the haddock fishery is increasing by leaps and bounds. The steam-trawler fleet has virtually doubled in the last two years, and nearly all of the fishing is done in a fairly restricted area. It is very urgent that data be collected as early as possible to show the effects of this increased exploitation on the population of haddock of that region. The flounder fishery has had a similar growth and needs similar attention. A serious effort is being made to collect statistics on the landings of these fisheries so as to provide an index of abundance from year to year, but such data must be accompanied by biological investigations if they are to be interpreted properly.

In the Middle Atlantic States the bureau has recently begun a program of investigations on the coastal fisheries from Long Island to Chesapeake Bay. This investigation is being so organized that pertinent data for population studies on the most important pound-net species are being collected and studied. With only one season's data at hand it is impossible to present conclusive results of this work. The life histories of the various species must be understood before the effects of birth rate, death rate, and migrations can be traced. The squeteague, or weakfish, is one of the most important fishes of this region and is receiving the most attention in the Middle Atlantic work. Thanks to the cooperation of the Bingham Oceanographic Collection, subspecific differences of the squeteague are being studied and will be of aid in this investigation.

Preliminary work has indicated some interesting complications. It appears that in the southerly localities only very small squeteague are ever taken in pound nets, while in other localities various sized fish will succeed each other as the season progresses. Various hypotheses will explain the phenomenon, but only continued investigation can establish the correct explanation. Meanwhile it will be very difficult to determine the age composition of the stock until this problem is solved. This phenomenon, whatever its cause, is significant in showing that in all fishery investigations great care must be taken to assure adequate sampling, both as to locality and time of season, and that the selective effect of different types of gear must be evaluated. The investigator in charge of this work is to be congratulated for having designed his sampling system to bring out this feature adequately.

Before leaving the middle Atlantic fisheries it should be remarked that the dominant species of the region is not receiving attention from the biologists.

This species, the menhaden, is taken annually in quantities exceeding the aggregate of all other fish on the Atlantic coast. An understanding of its fluctuations would be important not only to the menhaden industry but to the general problems of fish fluctuations of the region. The urgency of this problem can not be overestimated.

The mullet fishery is perhaps the most important on the South Atlantic and Gulf coasts. A study of subspecific difference has shown that in general the mullet of a particular region do not mix greatly with those of other regions. This defines the responsibility of individual States in the conservation of this fish. It was formerly the general opinion that North Carolina mullet went to Florida for the winter. This naturally removed all incentive to protect the fishery in either State, for each thought the other might thereby get the advantage. By defining the races of mullet the way has been cleared for State protection.

Recent fishery investigations in the coastal waters of Texas have resulted in a better understanding of the spawning and nursery areas of the more important fishes. A peculiarity of this region is the unusually wide seasonal variations in salinity of the lagoons, which become brackish in the rainy season and salter than the ocean in the dry season. Since this characteristic may be affected by man through the dredging of inlets, it is important to understand the biological consequences of these salinity variations. The studies of spawning areas and nursery grounds have paved the way for more concentrated work on the effects of varying salinity and temperature on the fish populations.

This outlines briefly the progress of the bureau's work on particular fish and fisheries. In summary, we are completing the descriptive work that must precede the quantitative work; work on some phase of population fluctuations is progressing on the cod, mackerel, smelt, mullet, and several important inshore species of the Middle Atlantic. There remain untouched more species than are being investigated, and many of these untouched problems are fully as important as those already under way. We need not be afraid of working ourselves out of our jobs.

I wish that I could present some new developments in theory of fisheries biology as the results of the recent Atlantic fishery investigations. Unfortunately this can not be done; but, instead, I wish to call attention to some interesting questions suggested by these investigations in the hope that through their discussion and the interchange of ideas in this conference there may result some advances in the theory and practice.

The problem of the good and bad years in fish reproduction interests me greatly. This phenomenon seems almost universal in the marine fisheries, though it is much more pronounced in certain species than in others. The Norwegian herring and the American mackerel seem to head the list of extreme contrast in good and poor reproductive years. The European plaice and the Pacific halibut seem to be at the other end of the scale, with but slight variations.

It is not unreasonable to suppose that if the cause of reproductive success or failure were known in the case of a single species it might pave the way for a more rapid determination in the case of other species, in much the same fashion as the first work on determination of age from scales has facilitated it in many other species, although each is a problem in itself and, as such, must be studied critically to assure correct interpretations.

This morning we were discussing the limit in the case of haddock population. I do not believe that the factor of the different phases in the life history of the fish was sufficiently brought out in the discussion. It seems to me entirely possible, and even probable, that the abundance of fish may be limited not by the amount of food available for the adult fish but by the amount of food available for larval fishes, for instance, or other physical or biological conditions that control the abundance of larval fishes that will be supported by the food present at the time when they have just hatched from the egg or at other subsequent times. The real limit of the fish population may be found in the weak link in the stage of fish growth rather than in the effect that might limit directly the abundance of mature fishes; and I think that establishing or at least getting some idea of what causes the ups and downs in a particularly fluctuating fishery like the mackerel is promising an entering wedge for the understanding of other fluctuations.

Another big question in the field of general biology is the interesting matter of survival factors. All existing species have somehow weathered all crises in their existence. As already pointed out, many species have great differences in

reproductive success from year to year. When a succession of poor years occurs the population must become much smaller, and a long enough succession of poor years could render a species extinct. In some way existing species have resisted all such periods. The mechanism of this resistance may be highly variable. In some species it may be longevity, by means of which there will still be mature fish left in the ocean to spawn even though no baby fish has been hatched for a number of years. In other species it may be the very prolific spawning of the few adults left. The mechanism, whatever its nature, is important in the theory of conservation, because overfishing during a critical period may seriously impair the mechanism in one case but have little effect in another.

In conclusion, a word should be said about the part which organizations other than the Bureau of Fisheries have contributed to the fishery investigations on this coast. Of most immediate value are the researches carried on by Canadian investigators at their St. Andrews station. These have not only contributed to the knowledge of the biology of fishes, but also to the physiological basis of marine phenomena. The studies of lethal factors and other of their physiological contributions will be of great value in the ultimate understanding, if there be such a thing, of the factors controlling the abundance of fishes. The work done by university research men in the various marine biological centers of the United States makes contributions in the physiology and embryology of marine forms. At the present time the extensive application of their findings may seem somewhat remote, but their true value to us will be realized when we ourselves have progressed farther toward an understanding of the basic causes of fisheries phenomena.

INTERNATIONAL HALIBUT INVESTIGATION

W. F. Thompson, *International Fisheries Commission*

When the International Fisheries Commission began its investigations of the halibut all that was actually known was the existence of a dangerous condition. Its staff had impressed upon it the need of formulating direct and practical measures for remedying this condition in the shortest time possible.

The temptation to begin, as in many another investigation, by general and diffuse biological studies was great. It would have been very easy to take it for granted that the only essential problems were those emphasized in the usual training of a biologist, such as the stages of the larval life-history. No doubt these problems would in the end have led us toward our goal, but to follow this trail many special fields would have to be entered upon, each of them requiring a skilled man and many years of work. One of the first questions to be met was, accordingly, as to the existence of some more immediately practical method of approach. This led directly to a searching consideration of what the problems at issue really were. It was then realized that no real problem had been defined.

The effort to define our problem led to a mode of attack which may be outlined here. This problem was really one of a change in the abundance of halibut, resulting from an artificial increase in the rate of mortality. A parallel could be drawn with the study of the mortality of mankind. To one undertaking such a study, diseases, anatomy, developmental history, and many other things are of interest. But those organizations directly interested in mortality, the insurance companies, proceed directly to a measurement and description of the rate of mortality and its effects without analysis of contributory causes and circumstances. They use great masses of data to overwhelm minor fluctuations and extend their observations over many years to determine the trend despite temporary fluctuations. This, then, was the rational mode of approach to our halibut problem.

The plan adopted was, therefore, first of all to study the vital statistics of the halibut. We planned to discover the statistical history of each section of the banks and the resultant condition. We planned to use this analysis and description as a basis and an indication of what problems of life history were essential to the passing of proper regulations, and we anticipated that when regulations were adopted their effect would be obvious only through a continuation of our studies of these vital statistics.

Last year, in a paper given here, the collection of these vital statistics was described, and it was shown how we had attempted, as the men in the insurance business had done, to make tables and curves of the changes in the

halibut stock and its fisheries. To do this we found a great deal of data that had been accumulated by intelligent men on the halibut vessels, and from this we were able to answer some of our questions. It was possible to measure the abundance of halibut by the catch per unit of gear, spoken of as pounds per skate.

Our first result showed that the different areas, or banks, were differently depleted. From Seattle northward each bank was characterized by a different level of abundance. When these levels were plotted we had a curve running from 50 pounds per skate near Seattle to 150 far north and west. But we found that this variation in abundance was the result of economic factors, not primarily of biological. It was a function of the distance from the markets, as the even nature of the change would indicate.

That this variation was a result of the fishery became evident when approached from another standpoint, the historical. Going back to the earliest records of the fisheries it was found that the original level of abundance of halibut was exceedingly different. On the older banks off the British Columbia coast it is now 50 pounds per skate, but it was once well in excess of 500. On the far northwest banks it has never been greatly in excess of what it is now. Were we to have the original curve of relative abundance it would, therefore, have been very different from that of the present. Where now the lowest level is found, there was once the highest. The commercial fishery has completely reversed the trend of the curve throughout the whole range of exploited grounds.

Incidentally this change in abundance was not the only result of the greatly increased rate of mortality on southern banks. The population was greatly changed as regards its composition, whether of ages or sex, and as regards its distribution on the banks.

Hence, it is evident that the southern banks show the greatest change, and it follows that there, if anywhere, it should be possible to study the nature of the decline in abundance, to apply, for positive results, our statistical method of approach.

The study of the decline on the more distant banks might be fruitful in the future, but as far as the past was concerned and as far as present available knowledge is to be obtained the older banks must be concentrated upon. Fortunately, I was already in possession of a great mass of records gathered prior to 1914, which extended as far back as 1902. It was possible to effectively supplement these by collecting records for the years since 1914. These were even more abundant that those for the earlier years.

From a thoughtful consideration of these data the essential problem has become evident. It is one that can be stated in terms of those vital statistics that have already been mentioned. We shall here seek to define this essential problem by means of a brief discussion of our results.

The record of relative abundance from year to year upon the older grounds off the British Columbia coast was determined in detail from the catch per unit of gear, corrected for changes. The catch fell from 300 pounds in 1906 to 30 in 1926, and in 1900 a few records showed 800 pounds. The fall during intermediate years was variable, but its trend was continuous and at the same rate.

For comparison with this rate of decline in abundance a record of the total catch taken year by year was desirable. Unfortunately no such statistics are available. The governments have given us a record of totals from all banks, but since the area of these banks has steadily increased the totals are meaningless except in so far as they failed to rise despite the vastly greater area exploited. Up to 1910, however, the total came from the older grounds alone. In that year the fishery was extended northward and to deeper water. From then until 1926 it seems impossible to reach any decision as to what the total from the older grounds actually was. In that year special inquiry showed that approximately 20,000,000 pounds was a fair estimate. We have concluded that the total catch on these grounds fell from 53,000,000 in 1910 to 20,000,000 in 1926.

This fall is a disaster in itself. It means that the fishery on the older banks is no longer a major one. But that the exact record of this fall is not available is greatly to be regretted for reasons that will be pointed out.

What brought about this great fall in abundance and total catch? From these two records the amount of gear used can be derived. The quantity of the catch divided by the catch per unit of gear should give the total number of units used, namely, about 177,000 in 1910 and 400,000 in 1926. Unfortunately, again, we have no direct records of this gear, from which the intensity can be

gauged, and our indirect calculation gives us no record for the years between 1910 and 1926.

From the work we are now doing it is hoped that the future course of these three curves—of abundance, of total catch, and of intensity—can be plotted accurately, for in their course lies the essentials of the problem we face. As I have said before, when the relationship of these three can be expressed in mathematical terms, and when we know the total yield to be expected from a given intensity, then regulation will become exact and scientific, not merely a step in the right direction.

A hint of the significance of the relationship can be gained from what we already know. The abundance of fish has fallen to 17 per cent—from 300 to 50; but the total catch has fallen only to 38 per cent, from 53 to 20 millions. In other words, the present low abundance is producing more, in proportion to the stock existent, than was formerly produced. There is an increasing resistance to the strain, which tends to bring the fishery toward stability. That is a result exactly in line with what has been observed in the North Sea; but there, as here, the exact history of the change is not accurately known because adequate statistics were not kept.

It is plain that were the intensity of the fishery to become a fixed quantity, the decline in abundance might go on only until the increase in resistance to the strain balanced the rate of removal by the fishery. In short, each different degree of intensity might have a stable corresponding level of yield, whatever it might be or however low it might seem. Then regulation would be concerned, for instance, with the question as to whether a low level of abundance with extremely high intensity would be economically better than a high level of abundance with a smaller fleet.

There is no real reason to believe, however, that any such uniformity in this increase in resistance will be found. The increase in resistance may all occur within the first stages of the decline in abundance, and thereafter abundance and total yield may fall together and in proportion to the increased intensity. This would imply that the maximum yield from the banks would be obtained at the time abundance was at a high level, and that subsequently increased intensity would actually be at the expense of the "principal," of which the yield should be the "interest."

To understand this better, it should be stated in terms of concrete details of life history instead of referring solely to these three graphs of intensity, abundance, and total yield. The theory embodied in these details is theory only, but the changes in these graphs which it may explain are definite facts.

Fisheries scientists everywhere have been interested in the speculation regarding the course of events in the plaice fishery. One of the theories advanced is that as the fishery develops and older fish are removed the younger have a greater chance of survival. Moreover, it is presumed that the rate of growth increases. In other words, the rate of inflow of new supplies of fish has increased. The *rate of replacement* is higher. It is supposed that the supply of food left for the young has greatly increased because not used by the old. With a higher survival rate and a faster growth, the resistance to the strain increases so far as it is able to replace the drain of the fishery. The rate of replacement finally equals outgo. Thus stated, this is a rather simple theory.

As applied to the halibut, it seems not so simple, but rather complex. The resistance to a strain and the rate of replacement cease to be necessarily uniform in their increase with declining abundance. For instance, the young fish are not relieved of competition from the adults because they have not had it to a great extent. They have always tended to school together and to inhabit their own banks. The fishery strips the older banks of large fish and leaves them barren and unproductive, so that the rate of replacement may not rise in response to diminishing abundance.

To strip a bank of its population is to pass far beyond the optimum conditions for productivity. It may, indeed, be profitable to remove some of the fish, but surely not so many as to leave the banks barren. In other words, the optimum conditions for survival of the fish have long ago been passed. Perhaps 10 or 15 years ago the banks were supporting their stock of adults to the best advantage, and if this is true the rate of replacement—the interest on the principal, if you please—will not increase, and any further decrease in abundance means the simple removal of the principal itself.

Then, to put the matter concretely, if 500 fish could exist under optimal conditions of growth and survival with a surplus of 50 fish, or 10 per cent, each year for the fishery, the problem is to allow the bank to retain that number. If the numbers are reduced to 100, with no greater rate of replacement than the above 10 per cent, only 10 fish could be taken by the fishery; but if that 100 should be replaced by nature at such a rate that 50 fish, or 50 per cent, could be taken annually, the population might well be allowed to decline to the 100 instead of the 500. It is the change in rate of replacement that is vital to the problem of regulation.

In our halibut problem, then, where did this rate of replacement, this resistance to a strain, begin to change? Where were the optimal conditions? We know little about it now because no one has kept accurate statistics. My plea this afternoon is that the rational course as to our fisheries consists, first of all, in getting statistics of sufficient accuracy over a sufficient period of time to give us an answer to these questions. What is the course of this rate of replacement? What will it be under regulation? We must express this in mathematical terms of abundance, of total catch, and of intensity (effort involved), for the rate of replacement is expressed by the total catch characteristic of each level of abundance.

In determining these statistical graphs or curves we have found in the halibut very great variations. As was said at the beginning, it is only over a period of years that we are able to determine the trends of these things. We can not pause to analyze the causes of all these variations; but we know that, as in the case of human statistics, the laws of mathematics prevail, giving stable averages and stable trends if enough data are accumulated.

To conclude, it has seemed to us obvious that the method of approach to the problem of depletion or overfishing is that of the insurance companies, to get accurate data over a period of years to enable us to describe these things in mathematical terms, taking into account the variation expected under the theories of error. Just as the insurance statisticians keep up their mortality tables and determine their trend from year to year, so must we with the halibut. We can not first analyze the natural causes of changes in this rate of replacement, for these are difficult and complex; but we must determine the rate and its changes and base our regulation upon it. We may not know why it acts as it does, but by the methods of the mathematician we may measure, just as do the insurance men, the constancy of the rate under the conditions met with. Such statistics will require years to secure, and they will be the better for an analysis of the natural history conditions underlying them. Upon such a program must action be based if scientific conservation is to come within reasonable time. In the meantime, sufficient can be discovered to guide us in the right direction, and with each succeeding year we will perfect our knowledge.

PROGRESS OF FISHERY BIOLOGY ON THE PACIFIC COAST

DR. WILLIS H. RICH, *In charge, Pacific coast fishery investigations*

The fishery investigations on the west coast deal primarily with the salmon, halibut, herring, and clam fisheries. You have already heard from Professor Snyder, and Mr. Thompson has told you of the work being carried on by the International Fisheries Commission. However, most of the salmon investigations on the coast—that is, in Alaska and on the Columbia River—have been carried on by members of the staff of the Bureau of Fisheries, and we have also been concerned with investigations of the herring and clam fisheries of Alaska; and just this year the bureau has undertaken an investigation of the oyster industry of the west coast under the very competent direction of Doctor Galtsoff.

I believe I am safe in saying that the primary object of most of the investigations is to discover the present and relative abundance of those several species of fish and shellfish that form the natural resources on which the fisheries are based and the proximal causes of such changes in abundance as may be demonstrated. Such a statement expresses very simply a problem that is tremendously complex, and the study of this problem leads, sometimes, into odd and unexpected channels. You have had illustrations of that put before you to-day.

In the first place we must determine the present abundance of fish of a given species, and this is primarily a statistical problem requiring extensive and accurate statistical data. The collection and collation of these data are, in themselves, tremendous tasks; but even when we have accomplished them we must ask ourselves what is a fair measure of abundance? Can we actually count or estimate the total number of fish of a species? I might say that can be approximated in some instances. On the other hand, is the total catch a good index that will give comparable figures year after year? Is the yield per unit of fishing effort a better index, more nearly comparable year after year? Or, if none of those measures can be applied satisfactorily, what can we use as a measure of abundance?

This first problem—to find a good measure of abundance—is, perhaps, the biggest and most difficult of all, and we may have to try several measures and try them out carefully, critically, experimentally, if possible, in order to determine which one is most reliable and best fitted for the particular problem in hand. R. A. Fisher, in his book on Statistical Methods for Research Workers states: "* * * from the modern point of view, the study of the causes of variation of any variable phenomenon, from the yield of wheat to the intellect of man, should be begun by the examination and measurement of the variation which presents itself."

It may be well to point out here that these studies in relative abundance serve not merely to show the present conditions of the fisheries, but must be continuously applied as long as we have any interest in our fisheries whatsoever. If a fishery is shown to be depleted and a remedy is applied it is just as important to discover the effect of the applied remedy as it was to discover, first, the fact of depletion.

Entering into this problem of the study of abundance, and second in importance only to the accurate measurement of the fluctuations, are the biological problems of migration and local races and the questions of dominant year classes, age at maturity, maximum age, rate of growth, and the like. These all bear more or less directly upon the question of abundance, and their solution will determine to no little extent the sort of measure of abundance we shall adopt, and will affect our interpretation of data bearing on the fluctuations in abundance. Without such data it will never be possible to distinguish depletion from the so-called natural fluctuations due to such causes as migrations, the presence of dominant year classes, etc.

Another important kind of investigation has to do with the causes of the natural fluctuations in abundance. While no sharp line can be drawn, it is important to distinguish the proximal from the more remote and fundamental causes. This may be illustrated by considering the effect of the presence of dominant year-classes. An abundance or scarcity of fish of a given species in a given locality may be due to the abundance or scarcity of fish of certain age groups that enter predominantly into the commercial catch. Such a cause of fluctuation is proximal and may be determined from such biological data as were mentioned a moment ago. Behind this, however, are all the various and more remote causes that may bring about the changes in abundance in the various age groups, such as favorable or unfavorable conditions of temperature during the spawning and incubation periods, and one might go on beyond that to the causes of the variations in temperature, the effect of solar cycles, etc.

Such consideration of the causes of fluctuations in abundance brings us inevitably to the general problem of limiting factors. What are the factors that limit the indefinite increase of a species? They are various in different species and, furthermore, vary from year to year. In the case of salmon, for instance, the capacity of the spawning grounds appears to be one of the most important natural factors limiting abundance; and this will vary greatly from year to year due to water conditions (just as an example we may say water conditions). In the case of many marine and fresh-water fishes, such as herring, halibut, and whitefish, the limiting factor may be temperature at spawning time or some other widespread oceanographic condition. Now, these important limiting factors may change in their intensity, their effect may be reduced or increased and the abundance of fish will be decreased or increased accordingly. There may be more than one limiting factor; in fact, there are sure to be many of them but of different values and operating at different stages in the life history of the species. If the most important factor were entirely eliminated, the species would increase until some other factor became operative and prevented further increase. Or, if a new factor comes to bear

upon the species, the abundance may be decreased to the limits imposed by this new factor. This is the effect of the establishment of a commercial fishery; it is of the nature of a limiting factor and its effect is proportional to its intensity. It would be possible to make some interesting speculations, based on the consideration of a commercial fishery as a limiting factor, but I shall not attempt to do that at this time.

Finally, there is a field of investigation that has to do primarily with the methodology of fisheries conservation. In the administration of the fisheries questions are constantly arising as to the best and most efficient methods for overcoming or preventing overfishing. Shall closed areas be established, and if so what shall be their boundaries? Shall restrictions on gear be imposed, and if so what shall they be? Or shall the fishing season be limited, and if so, how? These and other similar questions of the greatest practical importance to the administrator are constantly presented, and an early and satisfactory solution is essential to the success of efforts to conserve the resources. These problems are not necessarily all within the proper province of the biologist, but there are, more often than not, biological facts that must be taken into consideration in making a decision, so that the biologist is necessarily involved whether he will or no. An interesting example of this is a recent study made by Mr. Sette of the daily trap catches of certain traps in southeastern Alaska, on which has been based a complete reorganization of the several fishing districts that were established there for purposes of administration. This is a field that has not received a great deal of attention, perhaps because it is less interesting than the others; but from the practical standpoint it is so important that I look forward to interesting future developments in this field.

I shall now attempt to give you a very brief summary of progress in some of the fishery investigations on the Pacific coast during the past two years.

The herring investigations have been continued by Mr. Rounsefell and have dealt primarily with the herring of Alaska. Sudies have been carried on along the following lines: (1) Racial differences; (2) age determinations, based on scale and otolith examinations; (3) rate of growth; (4) condition—that is to say, the relative fatness; (5) migrations as determined by tagging experiments; and (6) statistical studies designed to show the trend of the fishery.

The racial studies were based on proportional measurements, vertebral and fin-ray counts, and have shown the existence of significant differences between the populations of virtually every locality that has been investigated. It has been demonstrated that the age of herring can be determined with a fair degree of accuracy from the scales, and the study of herring from a number of different localities has shown that dominant age groups are present. The variable success of spawning in different years may, therefore, be the explanation of most of the great changes in abundance of the Alaska herring, although it hardly seems adequate to explain the almost complete disappearance of these fish from some bays where they were formerly abundant. The age of most of the herring found in the commercial catches ranges from 4 to 12 years, with a maximum of 17 years. The scale studies also show that there are enormous differences in growth rate, 6-year-old herring from Dutch Harbor, in the Aleutian Islands, being 6½ centimeters longer than those from Stephens Passage, in southeastern Alaska. Taking these two localities as examples, it appears that the Stephens Passage herring require about 9 or 10 years to attain a size of 10½ inches, while those at Dutch Harbor attain a length of 11½ to 12½ inches in about 6 years. The studies of condition have shown that there are distinct differences in the time of year when the fish in different localities attain their best condition, and that there is also a difference in the condition attained in different years. The tagging experiments did not prove successful, so far as intermediate returns were concerned. No tags were recovered, although over 3,000 were attached in Kachemak Bay, one of the larger arms of Cook Inlet. This tagging was only done last summer and it is possible, of course, that returns will be secured during the next season. The statistical studies of the fishery have shown that there is a lack of continuity of the fishery in any particular locality and that there have been marked shifts in the fishing grounds. These facts, taken with the evidence of a decrease in the average size, indicate that there has probably been some depletion, but it can not be said that the evidence for depletion is clear.

An investigation of the razor-clam fishery has been conducted by Dr. F. W. Weymouth, of Stanford University, and Mr. McMillin on the beaches of Washington, and in several localities in Alaska.

Growth rates and length of life have been calculated for clams from each commercial bed. Racial studies proved that only one species was involved, and the growth curves are directly comparable. The Alaskan beds are recovering, due to present regulations; but the Washington beaches are seriously depleted, although the pack has not decreased. The digging effort has increased 1,000 per cent in 10 years, and a decrease in the catch per person has kept pace. The age has been reduced until 2 and 3 year old clams form the bulk of the catch where 6 and 7 year olds were dominent 5 years ago. Regulations such as are already in force in Alaska would restore the beds in a short time, and such measures are now before the State legislature of Washington.

In the case of the clam studies, those have been carried on as heretofore, although nothing was done during this past season on the clam beds in Alaska. In 1927 a survey of the clam beds was made, but the general investigation of the clams has progressed to a point where now it is not considered necessary to continue the detailed investigations that have been carried on before. In regard to the oyster investigations that are being carried on by Doctor Galtsoff and Mr. McMillin, the work this year has been merely preliminary and no definite results, of course, have been secured so far.

Turning now to the salmon investigations, which are, as you know, those with which I have been personally concerned and which are perhaps the only ones of which I have to speak much to-day, the salmon fishery, particularly that of Alaska, far exceeds in importance any other on the west coast, and naturally a correspondingly large proportion of our efforts is devoted to a study of the salmon resources and the salmon fishery. Furthermore, we have been charged with the care of the fisheries of Alaska and, as a result of this responsibility, it has been especially important to make our investigations such as will be of the greatest practical use in the administration of these fisheries.

The general program of the salmon investigations was outlined at the divisional conference two years ago and has since been published (Document No. 1029). No important changes have been made in this program, so that it will not be necessary to repeat it here, and I shall give you only a summary of the more important advances made during the past two years.

It is possible to report material progress in the work of assembling and collecting the statistics of the Alaska fisheries. Mr. Ball, of the Alaska division, has been working on this for nearly three years, and he and I completed last winter a section dealing with the salmon fisheries of Bristol Bay and the Alaska Peninsula. On account of the very local character of most of the fisheries on the peninsula it is impossible to give any general conclusions except to say that some localities show marked depletion while others do not. If you are interested in the details I refer you to the published report (Document No. 1041).

Certain facts of more general interest developed from the study of the red salmon runs in Bristol Bay. We were able to show that one of the important areas in Bristol Bay, namely, Nushagak Bay, is much more seriously depleted than the other sections. Perhaps I should say *was* more seriously depleted, as the run at Nushagak in 1928 was exceptionally good, contrary to expectations. It seems quite probable that this improvement was due to restrictions imposed on the fishery in 1924, resulting in an increased escapement, because our data show that the majority of the Nushagak fish are 4 years old.

We were able to show, also, from this statistical study, that there is a high degree of correlation between the red-salmon catches in Bristol Bay and in the Ikatan-King Cove-Shumagin Island district on the southern side of the end of the Alaska Peninsula. This corroborates the results of the tagging experiments conducted in 1922 and 1923, which showed that the fishery in the Ikatan-Shumagin Island district drew extensively upon the red-salmon runs of Bristol Bay. As a matter of fact, the evidence, I think, from our statistical data is even stronger than the evidence we secured as a result of tagging operations, although, of course, it is not so direct.

This analysis of the statistics of the Alaska salmon fishery is being continued and will ultimately include the whole of the territory. This winter we are working on the central district, including the fisheries from Chignik to Bering River. Throughout this study we have been compelled to make use of the total annual catch as our measure of abundance. It has proved impossible to get any more refined measure, although, as you all know, the total catch is seldom entirely satisfactory. We are attempting with fair success to segregate the data into

small geographical units and with less success to determine the amount of gear used in each locality. There are many difficulties, and the records are not always as complete as we should like, but believe that they show the important fluctuations in yield and the general trends. It will take several years before the study is complete, but I am sure that when it is complete we shall have a series of data of the greatest importance, both from the biological viewpoint and for the practical administration of the fisheries.

An important phase of our investigations is the study of the oceanic migrations of the salmon. This has an important bearing on the relation between the fisheries in different localities. As an example, I refer again to the study of the salmon runs of Ikatan and Shumagin, in which we showed by tagging experiments that the runs there were really a part of the runs of Bristol Bay. It is obviously important to know the relationship between runs of this sort, because, as in the case of the ones to which I have referred, it made a good deal of difference in our attitude toward the fisheries in Bristol Bay when we realized that the resources of Bristol Bay were also being attacked by the fishery at the end of the Alaska Peninsula. From a practical standpoint, if we were going to impose restrictions on the fishery in Bristol Bay it was necessary, of course, to impose as nearly equal restrictions as we possibly could upon the fishery at the end of the Alaska Peninsula.

Our study of these migrations by means of tagging experiments has not been as extensive during the past two years as it had been during the years between 1922 and 1926, partly for the reason that most of the major problems (general problems) of distribution or migration have been well worked out during the 5 years in which we were tagging extensively throughout the territory. At the present time it seems desirable to restrict our tagging operations to specific problems, and we are only using that method of study in cases where we have something very definite that we want to find out.

In 1927 the tagging operations were conducted mainly in southeastern Alaska and were designed to supplement the results of our previous experiments there where a few finer points needed to be worked out. The report has not yet been published.

In 1928 we had a little problem out on the Alaska Peninsula at one of the minor fishing localities, where, for other reasons, we had suspected that the red salmon taken also belonged to the Ikatan-Shumagin run, and we did a small amount of work there this last year. The results were not entirely satisfactory but did show some migration to Bristol Bay, although unfortunately it was performed so late in the season that we could hardly have expected very definite results.

One of our big problems in Alaska is that of ascertaining the normal return from known spawning escapements. You realize, of course, the general habit of the salmon of returning to the streams in which they are hatched. This fact is very well established for the red salmon, the king salmon, and the silver salmon, but in regard to the other two species of salmon, the pink and the chums, our information is very little; practically we have none. We are not sure to what extent the theory applies to these two species of salmon, and the remarks that I shall have to make here refer primarily to the red salmon and to none of the other species. The red salmon, we are convinced from plenty of evidence, do follow the parent-stream theory and return to the stream in which they were hatched.

The problem in regard to the return from known escapement hinges upon the number of spawning individuals necessary to maintain the run in any given stream. This problem also was outlined at a conference two years ago. At present the requirement demanded by law is for a 50 per cent escapement, but there is no assurance that this is the most desirable way of handling the situation. It is possible that in some years 50 per cent is not enough (we are quite sure that under certain conditions it is not enough) and in other years it may be too much.

There is another scheme, however, for handling this situation and getting into the streams an adequate escapement, which is probably better than the 50 per cent requirement, although it is extremely difficult to apply except in a very few places, and that is to demand a minimum escapement of a certain amount. That is being done at two or three of the streams in Alaska where the Bureau of Fisheries maintains counting weirs. For instance, at Karluk and Chignik, both, the requirement is not less than 1,000,000 fish to escape to the spawning grounds. I think this undoubtedly would be an ideal way of han-

dling the situation if it were possible to apply it generally, but that is entirely out of the question.

On account of the great interest that we have in this particular problem of the return from known escapement, we have selected a few particularly favorable localities, the two chief ones being Kariuk and Chignik, and are making special studies there. In these two streams weirs are being maintained, and the spawning escapement is counted through, so that we know exactly how many fish escape from the commercial fishery and go up to perpetuate the race.

In addition to that, it is necessary to very carefully sample the commercial catches, taking scales and measurements, so that we may determine the percentage of fish at each age that are captured at any given season. It is necessary to make adequate sampling of the commercial catch and to examine the scales of this adequate sample so that we may know, for instance, just how many fish in 1928 came from the spawning of 1923, how many came from the spawning of 1924, and how many came from the spawning of 1922, and so on. Then, once we have those data over a period of years, it is possible to show just how many fish, we will say, came from the escapement of 1922. But because of the fact that the percentages fluctuate at different seasons it is necessary to have adequate sampling throughout the season. Every week, for example, we should have a large enough sampling so that we can determine the percentage of fish of the different age groups that were taken in the commercial fishery in that week. Thus, we have the data of the escapement and over a period of years the number of fish returning from that known escapement as 4 or 5 or 6-year-old fish as the result of our sampling and scale examinations.

At Chignik we encountered a number of difficulties, and it will require a lot of detailed work over a number of years before we shall probably be able to analyze the situation there in a way that is satisfactory and the way that we are able to do fairly well at Karluk. There are extremely difficult problems in age determination from the scales of Chignik salmon; and, furthermore, it is apparently true that there are certain fluctuations in the percentage of fish of different age groups. You may have in one week, for example, 75 per cent of 4-year fish, and in the next week you will have 75 per cent of 5-year fish and the rest of them 6-year fish.

At Karluk we do not have sudden changes in age composition of the runs. We have gradual changes that extend over the season but nothing of the sort that Mr. Holmes has found in the case of the Chignik fish. This made sampling more difficult, for to get an adequate sample we must have more fish, a big enough sample every week (theoretically we ought to have it every day) so that we could determine for that particular day how many 4 or 5 or 6 year old fish were taken in the commercial catch.

The first year in which the weir was established in the Karuk River was 1921. We got 4-year fish back in 1925 and the 5-year fish back in 1926 and 6-year fish back in 1927. There are a few 7-year fish in the Karluk run, but they are not in sufficient numbers to be of much importance. We can, for practical purposes, say that the important ages there are 4, 5, and 6 years.

In 1927 we had all of the returns from the first two years of operating of the weir—1921 and 1922. We got our 4-year fish back in 1926, 5-year fish in 1927, and 6-year fish in 1928. Now, for all practical purposes, we have two years (1921 and 1922) on which to calculate the percentage of returns from the known escapements. We have most of the returns from the 1923 escapement, but we shall have to wait until the end of the 1929 season before we shall have all of that; and from now on, every year will add just one more figure to our series of data.

It will take a long time before we have enough data to determine the trend, the sort of returns, that we can expect from a known escapement. We have been able, however, with what little we have at Karluk and from examination of the data we have at Chignik, some of which applies to this problem, to satisfy ourselves fairly well that the relationship between escapement and future run follows a regular curve, which rises very rapidly and then slopes off to the horizontal. For example, the return from about a 500,000 escapement has been approximately 2,000,000 fish, and the return from about a 1,500,000 escapement has been about 2,500,000.

Just what will happen as the escapement increases I do not know. The return will reach a maximum, perhaps, and then it is conceivable that it may

come down. In other words, it is possible that there is such a thing as overstocking. That is a point that we are not sure of, and we do not like to say very much about; but there is this much to be said, that in the case of the Chignik salmon there is some evidence of such a thing as overspawning. For instance, in 1927 there was a tremendous run of pink salmon into the Karluk River. The story is almost too much to tell you about, but a rack in the Karluk River was blocked up and washed out with the bodies of these pink salmon that came down there. Probably 4,000,000 or 5,000,000 pink salmon spawned in perhaps 15 or 20 miles of the river, and the decayed and dead bodies were sufficient to take out of the water most of the oxygen (perhaps all of it), with the result that the adult pink salmon that were perfectly normal died. They did not die from injuries or anything of that sort, but they just simply died; and also the small salmon in the river died, and little fish up the river died, and the bullheads died, and the humpbacks died. The next year the return was practically nothing; it was 15,000 or 20,000 fish, whereas in 1924 the commercial fishery took in the neighborhood of 2,000,000 salmon, and there was an escapement into the stream estimated at 4,000,000.

At Karluk we have also another interesting problem dealing with the fluctuations in the rate of mortality. The rate of mortality is, of course, one of the important factors that determines the productivity and relative abundance of a species; and at Karluk we were interested not alone in the discovery of the rate of mortality but in determining something about the fluctuations in the rate of mortality in fresh water as compared with the fluctuations in the rate of of mortality in fresh water as compared with the fluctuations in the rate of mortality in the sea. The question was whether the greatest fluctuations in mortality occur in fresh water or in the sea. If they occur in fresh water it is possible that by means of hatchery operations, or something like that, we can decrease that rate of mortality; but if the greatest fluctuations occur in the sea, it will be much more difficult, if not impossible, to control them.

In attacking this problem we have undertaken a series of marking experiments, and we have marked about 50,000 fish every year since 1926. In that year we did not quite make 50,000, but in the last two years (1927 and 1928) we marked 50,000 of them each year. In the case of the Karluk fish the young remain in fresh water, most of them, until they are in their third year. For instance, fish that are spawned in 1925 in the fall of the year will not go out to sea until the spring of 1928.

In this marking experiment we are trying to determine the number of seaward migrants. We are doing that by very careful sampling to determine the number of marked fish that are taken in the commercial catch. Then, knowing this, it is a relatively simple matter of proportion to work out the total number of marked fish in the commercial catch, and from that the total number of seaward migrants. The matter is more or less complicated by the fact that there are several age groups reported and by the fact that we have several experiments represented. For instance, next year we will get a return from the markings of 1926, 1927, and some from 1928. The whole thing is rather complicated, and it is hardly worth while to go into details at this time; but it is sufficient to say that we can, by means of our sampling methods, determine with what we think is considerable accuracy the number of seaward migrants. Of course, it is far too early to say anything about what are the fluctuations in the rate of mortality. We have fairly complete returns now from this one year from our markings of 1926, but that, of course, is not at all sufficient.

There are several interesting points in connection with this experiment, however. One is the fact that the actual percentage of marked fish returned has been very large, higher than anyone had ever imagined it would be. We have made extensive marking experiments on the Columbia River since the beginning of 1916, totaling for both chinook and sockeye salmon 2,000,000 since 1916. The best results we have in any of those experiments on the Columbia River indicated about a 5 per cent return. But in the case of our Karluk salmon, in which we marked the fish that were in their third year (that is, two years plus of age), our returns already are approximately 25 per cent; and after we get our 6-year returns next year I feel quite confident that the returns of marked fish will be in the neighborhood of 30 per cent, or perhaps a little over. That means that one out of every three of the fish we have marked is coming back; one out of every three of the seaward migrants is going to survive and come back as an adult fish ready to spawn. That is unexpected in the case of salmon, and I do not know of any evidence of survival as big as that in any of our fishery investigations.

In 1926 we split our experiment into two parts in order to get some sort of empirical measure of the probable error that was involved. We marked that year approximately 47,000 fish. Twenty-five thousand were marked by the removal of the right ventral fin; 22,000 by the removal of the left ventral fin. This last season we found that our percentage of return in the two halves of that experiment was very close. They were, I think, within about one-half of 1 per cent of one another, affording good evidence that our method of handling of the seaward migrants and our method of sampling the commercial catch for marked fish is adequate.

I do not want to leave the discussion of the fishery investigations of the Pacific coast without first paying such poor tribute as I can to the man who, more than anyone else, is responsible for our gathering here to-day. The sudden and untimely death of Dr. Charles Henry Gilbert has taken from us a real leader; one, from whom many of us, either directly or otherwise, received inspiration for research and interest in fishery investigations. It was he who, in 1909, first saw, with characteristic clear-sightedness, the possibility of developing a new branch of ichthyology, and who first brought his remarkable abilities and tireless energy to bear upon the great scientific and practical problems of our fisheries. It can truly be said that he was the founder of modern fisheries science in America.

His own published contributions to this new science were many, but the full measure of what he did can, perhaps, only be appreciated by those of us who were privileged to work closely with him and so to know his great influence and the great aid he gave to the younger men with whom he came in contact. His method of working was always direct; he was concerned primarily with the discovery of facts and not with the development of theories, and his keen, analytical mind was always seeking to select the important from the unimportant, the pertinent from the irrelevant. He realized more clearly than most that his and our observations and studies were only the beginning of a science, and he would not permit himself to publish speculations as to the ultimate causes behind the observed facts. What we need, he thought, is more facts and still more facts; then, in time, out of an abundance of facts, it may be possible to build theories that will explain; but until we have abundant facts he, at least, would not theorize. Some of us shall always regret this, athough it was so fundamental a trait with Doctor Gilbert and one so intimately associated with his analytical faculties; but we know that he thought deeply upon the fundamental principles of the biology of the fisheries and, had he permitted himself expression, his ideas would undoubtedly have been most stimulating. It is left to us to carry on the work that he started, and I, for one, hope that I may be able to carry my small share of the burden in a way that would deserve his approval were he here to give it.

PROGRESS OF FISHERY BIOLOGY ON THE GREAT LAKES

Dr. John Van Oosten, *In charge, Great Lakes fishery investigations*

Under the subject "Progress of fishery biology on the Great Lakes" I have been asked to review with you the progress of the entire science in the Great Lakes area, whether the result of bureau investigations or of investigations conducted by private institutions. It is, of course, utterly impossible for me to review all the work that may properly be included in the category "Fishery biology on the Great Lakes." This would involve a consideration of at least 85 scientific papers that deal specifically with the fishes of the Great Lakes proper, of some dozen papers on the parasites of Great Lakes fishes, and at least 60 scientific publications on the food of the fish, on the plankton, and on other subjects that may properly be included under the limnology of the Great Lakes. I must, likewise, ignore the surveys on depths, etc., conducted by the War or Navy Departments of this country and of Canada, and the expeditions of local character or of very limited scope, as, for example, the Shiras expedition to Whitefish Point, Lake Superior.

Time permits only a passing mention of the numerous statistical surveys conducted by the national and local governments of both Canada and the United States. These statistical surveys, 10 of which were conducted by the United States Government, are of immense value to the fisheries. The earlier statistical reports of the United States Fish Commission included, in addition

to purely statistical material, valuable information on the history of events that influenced the development of fishing in each important locality on the Great Lakes, on the location of fishing grounds, on the natural history of the more valuable market species, and on methods for the improvement of the fisheries.

We shall confine our review, then, to the more important general surveys that come within the scope of fishery biology and shall consider only those surveys that were conducted on the Great Lakes proper, its connecting waterways, and Lake Nipigon. It seems best to review the subject under three headings—(1) limnological surveys, (2) fishery surveys, and (3) fishery regulations.

Limnological surveys.—Limnological investigations in this country have been restricted almost entirely to small inland lakes, while the Great Lakes have received very little attention. Probably the first paper that deals with the general environmental conditions of the Great Lakes is that of Agassiz (1850) on the physical character, vegetation, and animals of Lake Superior. It was not until 1893 that the first real limnological investigations began on the Great Lakes, and it is interesting to note that economic pressure gave the impetus to this first limnological study. Reighard, in the introduction to his report on "A biological examination of Lake St. Clair" (1895), called attention to the marked decrease in the catches of whitefish in the Great Lakes. This condition continued in spite of the large output of the fish hatcheries, and it was with the hope of determining where in the Great Lakes were to be found the conditions favorable to the whitefish fry that the Michigan Fish Commission established a scientific laboratory on Lake St. Clair in the summer of 1893. The staff of eight men undertook an extensive study of the fauna and flora of the lake and also a study of the food habits of the fish.

Perhaps the greatest significance of their study is that it was the first attempt by American biologists to use methods worked out by Europeans for the determination of plankton productivity of a lake. It also resulted in a great increase in the knowledge of the natural history of our fresh-water flora and fauna. The facts learned were made use of in recommendations for the better planting of whitefish fry in Lake St. Clair.

A year later, 1894, a more detailed study was made of Grand Traverse Bay in Lake Michigan under the direction of Dr. Henry B. Ward. Particular attention was paid to the kind, source, and abundance of food for the whitefish. The investigation showed, in the words of Ward, that "there is a plentiful supply of whitefish food on the fishing grounds. No reason can be assigned for the diminution in the supply of whitefish save overcatching." In closing his report, Doctor Ward emphasized the necessity of having the continued services of full-time investigators to insure the collection and publication of reliable data. It is needless to add that this suggestion fell on deaf ears.

The next survey of interest to us dealt with the pollution of boundary waters and was conducted during the years 1912–1914 under the auspices of an international joint commission. This survey attempted to determine "to what extent and by what causes and in what localities have the boundary waters between the United States and Canada been polluted so as to be injurious to the public health and unfit for domestic or other uses." According to the United States Tariff Commission's report on lake fish (1918), some 19,000 bacteriological tests were made, and these indicated that "although the great body of lake water itself is for the most part remarkably pure, in certain portions of the Great Lakes and in all the connecting waterways serious pollution was shown to exist" (p. 8). The western end of Lake Erie, west of a line drawn from Point Pelee to Sandusky, was most noticeably affected. This area comprises the most extensive spawning grounds of fish in Lake Erie. It must be explained, however that the studies of this commission were made primarily with the view of defining the water areas unsafe for domestic use and not with the view of locating the areas unsuitable for fish life.

Since 1921 the Ontario fisheries research laboratory of the department of biology, University of Toronto, has made extensive studies on the fishes, the hydrography, the plankton, and bottom organisms of Lake Nipigon. Particular attention was given to the productivity of the lake in fish food, both planktonic and benthonic.

Previous to 1926, then, only three limno-biological investigations were carried on in the waters of the Great Lakes proper or its connecting waterways and Lake Nipigon. There have, however, been numerous important contributions made to our knowledge of the productivity of the Great Lakes. Among these may be mentioned the work of Vorce (1880, 1881, 1882), Forbes (1882, 1891), Thomas and Chase (1887), Turner (1892), Woolman (Report of the United States Commissioner of Fisheries for 1897 and 1898), Snow (1904), and Eddy (1927) on the plankton, and the extensive studies on the flora and fauna of Georgian Bay by various investigators from the Georgian Bay biological station of the Biological Board of Canada. Special mention should be made of the reports on the different forms of nitrogen found in the water of Lake Michigan by Domogalla and his associates (1925, 1926). In addition to these studies, more than two dozen publications that deal with certain phases of the limnology of Lake Erie or the ecology of its fauna have appeared from the Lake laboratory and its successor, the Stone laboratory, established by the Ohio State University on Lake Erie.

The most recent series of limnological surveys on the Great Lakes had its inception on Lake Erie in 1926, when "during the month of August the division of fish and game of the State of Ohio undertook to learn how far out from the lake-shore cities sewage pollution may occur in sufficient quantity to affect the life of fishes, either directly or by injuriously modifying feeding or breeding conditions. The evident reduction of the commercial fishing and angling * * * in recent years, * * * the dispute as to whether netting or pollution is the chief factor involved, made such a survey appear an urgent necessity." (Osburn, 1926.) A scientific staff of five was maintained. From the preliminary study of the bottom samples, plankton tows, bacteria, dissolved oxygen, hydrogen-ion concentration, and temperature it was concluded that pollution serious enough to menace fish life was confined to the sewer outlets from cities and to the rivers, the inclosed bays or harbors, and their immediate environs, into which this sewage was dumped. "There was no serious contamination of the open lake in deeper water, or very far from sources of pollution in shallower waters along shore." (Osburn, 1926.)

In cooperation with the United States Bureau of Fisheries, the State of Ohio resumed its limnological investigations of the western end of Lake Erie in the summer of 1927, employing two permanent investigators.

It was not until February, 1928, however, that the most complete and the most extensive limnological survey ever undertaken on the Great Lakes System had its inception, when, at a conference called by the United States Commissioner of Fisheries at Cleveland, Ohio, the various biologists working independently or as members of the staffs of research institutions and universities on Lake Erie formed the Cooperative Fishery Investigations in Lake Erie. In accordance with the plans discussed at this conference the survey was carried on by two parties, one at the western and the other at the eastern end of the lake. The work at the western end of the lake was carried on under the auspices of the State of Ohio division of fish and game, with a staff of 5 men stationed at Put in Bay. A staff of 13 men, under the joint auspices of the Province of Ontario, the Buffalo Society of Natural Sciences, the New York Conservation Commission, the health department of the city of Buffalo, and the United States Bureau of Fisheries, covered the eastern end of the lake.

While the two parties worked independently of each other, both attacked the problem with the same objective in view—to learn the physical, chemical, and biological conditions that exist in the lake, with particular reference to their bearing on young fish.

Under the head of physical conditions is considered topography, morphometry, bottom deposits, waves, currents, transparency, and temperatures. These factors are quite different at the two ends of the lake and affect, in important ways, both the chemical and biological conditions. They also necessitated the use of different equipment and methods by the two parties.

One of the main points of contention with regard to the depletion of the Lake Erie fisheries has been the relative importance of pollution and overfishing. As a result, considerable attention was paid to the kinds, extent, and effects of pollution. The investigation showed that there were localized areas where trade and domestic wastes caused considerable contamination of the waters, but in no case did this extend far from the source. This was indicated by chemical and bacteriological tests and by examination of bottom deposits for putrescent organic matter.

Determinations of dissolved oxygen, of free,. fixed, and half-bound carbon dioxide, free and albuminoid ammonia, nitrates, and of hydrogen-ion concentrations were made at numerous stations. With the exception of the polluted areas the lake was found to have no chemical conditions considered detrimental to the normal existence of organisms.

A study of the plankton showed an abundance of both plants and animals, which form the ultimate source of food for fishes. Certain plankton collections were made with meter nets, and these were also used for taking young fish, in addition to the trawls used on and near the bottom. In all, several thousand fish fry and fingerlings were collected. Seine hauls were made on the New York and Ohio shores to determine the abundance of the various species of fish in the various kinds of habitats.

In general, the results of these investigations show nothing in the physical, chemical, and biological conditions of Lake Erie to explain the present depleted state of the commercial fisheries.

At the second conference of the Lake Erie investigators called by the United States Comissioner of Fisheries at Columbus, Ohio, on February 3, 1928, it was planned to continue the limnological work in 1929 and to extend it in order to obtain more complete data for the entire lake.

Fishery surveys.—The surveys devoted specifically to the fisheries of the Great Lakes have been numerous; eight of these, excluding the statistical, have been of sufficiently broad scope to warrant consideration here.

The first general survey of the Great Lakes fisheries was made in 1871 and 1872 by James W. Milner, assistant commissioner of the United States Commission of Fish and Fisheries, who visited many fishing localities, gathered testimony of the fishermen as to the condition of the fisheries, its comparison with former years, the kinds of nets in use and their effect on the numbers of the fish, and the opinion of the net owners on the influence of protective legislation. He also acquired information on the modes of fishing, the species captured, some knowledge of their habits, and the general distribution of invertebrate forms in certain areas of the lakes.

Milner's discussion on the causes of decreases of the food fishes is highly illuminating in view of our present investigations of depletion. He believes that overfishing is a secondary factor, but that the capture of immature fishes is, without doubt, the principal cause of decrease. He discusses the various causes cited by the fishermen, such as the destruction of small fish by pound nets, the wasteful method of fishing by gill nets, the dumping of offal on fishing grounds, and pollution—causes still cited by the fishermen of to-day.

Milner's discussion of proposed protective legislation likewise could be bodily incorporated in a 1928 report with slight modifications. For example, whereas we are discussing at the present time the desirability of abolishing the trap nets, Milner did likewise with respect to the pound nets. We are discussing the merits and demerits of the sorting process; so did Milner. We shall recommend a larger mesh for crib nets; Milner did likewise. We can recommend, with Milner, that no gill net mesh of less than 4½ inches should be used for whitefish. Again, we believe, with Milner, that "a law restricting the number of pound nets to the mile along the shores of the lakes would be a favorable regulation."

I have stressed Milner's admirable report of 1874 to impress you with the fact that even some of the simplest problems that confronted the fishing industry more than 50 years ago have been allowed to pass unsolved and still face the fishing industry to-day. Milner expressed little confidence in the adequacy of protective legislation but believed that artificial propagation is the only certain salvation of the fisheries. The author's notes on the natural history of the important commercial fishes are unusually complete, certain data, as, for example, those on growth rates, being even now the best available.

The most extensive and most complete survey of the Great Lakes fisheries was that conducted in 1893–1896 by Rathbun and Wakeham, appointed as a joint commission by Great Britain and the United States to study the fisheries in waters contiguous to both countries and to consider and report on the regulations, practices, and restrictions proper to be adopted in concert on the (1) limitation or prevention of exhaustive or destructive methods of taking fish, (2) the prevention of pollution or the obstructing of waters to the detriment of the fisheries, (3) the adoption of uniform close seasons, and (4) the adoption of practical methods of restocking and replenishing the waters with fish.

In their excellent report the authors discuss, for each body of water, the statistics, history of the fishing methods, and extent and causes of decrease, and present excellent notes on the distribution, movements, size, and spawning of the important market species in each lake.

Another refreshing feature of this report is the very specific recommendations given for each lake. In many reports recommendations are stated in such general terms that the investigations carried on to arrive at them hardly seem to have been warranted. The specific recommendations proposed by this commission, though excellent conservation measures, were highly restrictive and consequently were not and some have never been adopted by the States. Two of the four general recommendations are highly interesting in view of the present widespread discussions on the Great Lakes concerning the administration of the fisheries and uniform laws. The authors write:

> Our observations, moreover, have clearly demonstrated the inexpediency of attempting to regulate the fisheries herein discussed by a rigid code of enactments, owing to their constantly changing character and conditions, and we would therefore urge, in the event of joint action by the two Governments, that a permanent joint commission, to be composed of competent experts, be provided for, which shall be charged with the direct supervision of these fisheries and shall be empowered to conduct the necessary investigations and to institute such modifications in the regulations as the circumstances may call for from time to time.
>
> That a uniform system of regulations common to the entire extent of each body of water along the boundary line is required to insure the protection of its resources is fully shown by the conditions which we have found to exist in nearly all of them, whether bordered on the side of the United States by a single State or by several States. The failure to secure adequate results in that direction has naturally been due to the diversity of legislation, but it has resulted in large part from the general lack of accurate information regarding the habits of the several fishes to serve as a basis for intelligent action. While we have been able to establish some important facts respecting the natural history of the commoner market species, much more remains to be accomplished in that regard, and the subject should be given due prominence in the future.

The next Great Lakes survey of interest to us is that conducted by the International Fisheries Commission in 1908 under the direction of Doctor Jordan. This survey was instituted primarily to acquire data for the preparation of a code of international fishery statutes to supplant those of the Canadian Provinces and of the various States. I have been informed that many data on the Great Lakes fisheries have been accumulated by this commission, but that no report on these has appeared in print. The attempt to place the Great Lakes fishery under national or international control again failed.

The survey conducted by the Biological Board of Canada on Lake Erie during 1920 dealt primarily with specific fisheries problems concerning principally the variations in the sizes of the ciscoes in the various sections of the lake. However, as the result of this survey several preliminary papers appeared on the growth of the more valuable market fish, including the ciscoes, whitefish, blue and yellow pike, and the perch.

In 1925 the United States Tariff Commission ordered a survey of the Great Lakes fisheries with a view of acquiring "factual data that may be useful in constructing a tariff schedule for fresh-water fish." Very valuable and new information was gathered on the status and cost of production of the various market fish by various kinds of gear in both Canada and the United States. The production of 1924 served as a basis for discussion. Detailed data are given on the methods and cost of fishing, of boxing and icing fish, of freezing, salting, smoking, filleting, and storing fish, and of transporting fish.

Dr. Walter Koelz (1926) summarizes the most recent knowledge on the fishing conditions of the Great Lakes, or, as he states the purpose of his paper (Bureau of Fisheries, Document No. 1001, p. 554), "to picture, with a historical background, the condition of the fisheries of the Great Lakes as they are today and to call attention to the need of intelligent action to preserve those fisheries." This author has had many years of experience on the Great Lakes, and in view of all this experience it is interesting to note that he finds the Great Lakes situation so complicated that it is impossible for him to state specifically how the production of the Great Lakes fish should be regulated. He writes (p. 613): "What form such legislation should take the writer can not say." Permit me to quote from his paper the last group of recommendations for the Great Lakes fisheries that have appeared in print and that have been based on extensive observations:

> 1. The needs of the Great Lakes fisheries should be studied, and in the light of the knowledge gained regulations having for their object the conserving of the fisheries should be created. We already have data to show that no single law can be devised to meet the

varying conditions presented by one lake, to say nothing of applying one law to several of the lakes. The application of any laws found advisable must be independent of political boundaries. The present division of authority over the fisheries among several States impedes the enactment and complicates the administration of any legislative provisions, and it is therefore urgently recommended that some definite and responsible organization, international in character, be provided through which a coordinated control of the fisheries may be secured.

2. The closed season to protect spawning fish should be restored wherever practicable, and no spawn should be collected if investigations and experiments fail to establish the desirability and effectiveness of propagation.

3. Investigations to determine the life histories of the important species already begun should be continued, and statistics reflecting the condition of the fisheries should be collected from year to year to supplement these studies. Only by means of such statistics interpreted in the light of life-history facts, can the fishing industry be intelligently controlled.

At the present time a general survey of the fisheries is being conducted on Lake Ontario by the Ontario fisheries research laboratory of the department of biology, University of Toronto, especial emphasis being given to the natural history of the coregonids.

The bureau's Great Lakes program is confined largely to an exhaustive study of the fisheries of Lake Erie After concluding a preliminary survey of the Great Lakes fisheries in 1927, it was realized that although there was urgent need for an investigation on Lakes Michigan and Huron, by far the greatest need existed on Lake Erie, where the cisco, one of the most valuable species of the Great Lakes, is facing commercial extinction. Our Erie survey began in August, 1927, with a staff of three men. In 1928 our personnel was enlarged to five scientific investigators and two assistants in the office—a staff of seven.

In our investigation we started out with a firm determination to ascertain just why, in the face of serious depletion of so many species of fish and the virtual extinction of the ciscoes in Lake Erie, it has been impossible to adopt a few of the excellent recommendations that have been discussed repeatedly in past years. I believe that in this respect our scientific investigation differs from most of the previous scientific surveys. We have attempted from the beginning to ascertain from the fishermen what, in their opinions, were the urgent problems to be solved first, and have shaped our program in part in accordance with their suggestions. We are convinced that it is absolutely essential to consider in our program the views and requests of the practical fishermen, for we believe that it is primarily the dissension among the fishermen that has blocked the passage of many excellent regulatory measures on the Great Lakes. We are convinced that under existing conditions our only hope of securing proper regulations for the fisheries of the Great Lakes lies in an impartial investigation of all the questions of dispute, irrespective of whether or not they have scientific value or fundamental significance.

It is for these reasons that much of our work at the present time deals with experimental fishing gear. The fishermen ascribe the depletion of the fisheries in part to the destructiveness of the various kinds of gear. Our first tasks, then, have been to ascertain the destructiveness of the commercial gill net, the bull net, the trap net, the pound net, and the seine to the various species of fish, at various seasons of the year and in various areas of the lake. In addition, we fished a series of experimental trap nets and gill nets of various sized mesh throughout an entire year in order to determine whether it is possible to fish the present types of gear with less destruction to the immature fish than occurs at the present time. We have not been entirely unmindful, however, of the scientific call within us. We realize that among other things the aim of fishery science is the detection and prevention of overfishing and the detection and prediction of fluctuations in abundance due to natural causes.

The fisheries of the Great Lakes differ in many respects from those of marine waters. They are confined to smaller bodies of water, and the detection of depletion is much more readily accomplished in these waters than in the oceans. Thus, without any critical or exact statistics such as are required for the detection of overfishing in the seas, we can assert positively that the sturgeon of all the Great Lakes, the bluefin of Lake Superior, the blackfin of Lake Michigan, the bloater of Lake Ontario, and more recently the cisco of Lake Erie are virtually commercially extinct. In so far as these fisheries are concerned we need not hesitate to recommend complete protection, although in certain cases it will be difficult of execution.

We believe that the problem of the nature of overfishing is one of the most important on the Great Lakes and that its solution is impossible without ade-

quate statistics. I have stated repeatedly to the fishermen and to conservation officers that adequate statistics is the most important single tool in the hands of a fishery investigator. Without it we can not ascertain the exact condition nor the real trend of a fishery; we can not detect nor follow nor explain the fluctuations in the actual abundance of the fish in the lake; we can not hope to foretell any great fluctuations in future yields nor have any means of checking the effectiveness of any new restrictive regulations that may be imposed on the commercial fisheries of a lake.

We have labored constantly to induce the States that border the Great Lakes to inaugurate a new system of collecting fishery statistics, whereby the biologist shall be enabled to compute the yields per unit of gear or effort and per unit of time. It is gratifying to announce that the new system has been established already in Michigan, New York, Pennsylvania, and Illinois. It is confidently expected that the other States and the Province of Ontario will follow their example.

The problem of fluctuations in natural supply due to natural causes is being attacked by the usual methods familiar to most of you—the analysis of the catch by year-classes, the determination of age and growth rates, the study of migrations, and the study of races. Efforts were made to collect all possible data on the biology and habits of the important commercial species, nine in number.

Fishery regulations.—On the basis of our studies we hope to be able to draw up specific regulations for the Lake Erie fisheries. We believe that these recommendations should not only be made with a view of conserving the fisheries but should be practical, enforceable, and reasonable. Without the latter qualifications they can not pass the legislatures, nor would they prove of any value. These latter qualifications make our task most arduous, for the human element must be considered. Our hope of success lies principally in bringing together the various factions among the fishermen and in cooperating closely with the departments of conservation in the various States.

This cooperation is likewise essential in our efforts to obtain certain uniform regulations, not only for Lake Erie but for each of the Great Lakes. Strenuous efforts have been made during the past two years to have the States and the Canadian Government accept a uniform code of statutes. At a conference called by Gov. Vic Donahey, of Ohio, at Columbus, Ohio, on February 16, 1927, to which were invited representatives from all the governments that border on Lake Erie and from the United States Bureau of Fisheries, there was formed the Lake Erie International Conservation Council, and resolutions were adopted. So far as I know this council and its resolutions died at birth. The International Fisheries Conservation Council of the Great Lakes, called into existence on March 3, 1927, at Lansing, Mich., by Gov. Fred W. Green, of Michigan, fared considerably better. This council met twice after its formation—on February 8, 1928, and again on December 5, 1928. Recommendations were adopted, but whether they will be enacted into laws by the various legislatures that meet this winter remains to be seen. We can at least point to one important concrete result of these conferences—the inauguration of the new system of collecting statistics in New York, Pennsylvania, and Illinois.

With respect to agreement on uniform fishery laws on the Great Lakes, I feel that history will repeat itself. Permit me to review briefly some of the attempts made to establish uniform fishery regulations on the Great Lakes.

As early as 1879, Jerome, of the Michigan fish department, recommended Federal control of the commercial fisheries of the Great Lakes. Since this time the question of uniform fishery laws has been discussed repeatedly, and several conferences have been called to secure action. In his report of 1890 J. W. Collins, of the United States Fish Commission, called attention to the need for uniform laws on the Great Lakes. His statements, which are to the point and apply with equal force to-day, are as follows (p. 17):

> The marked diversity in the laws regulating the fisheries of the States bordering on the Great Lakes is a matter which appears to deserve consideration. The desirability of having some cooperative action on the part of the various lake States would seem to be apparent, in order that legislative enactments might have an equal bearing and influence upon the fisheries and the fortunes of the fishermen. Much dissatisfaction is often expressed by the latter concerning the inequality of laws governing the fisheries in regions immediately adjacent. It is only natural that the fisherman who finds his work interrupted and his enterprise defeated by restrictive measures should feel discontented when he sees his neighbor just across the State line prosecuting a similar fishery without hindrance. It is also, perhaps, an open question if any permanent benefit can be obtained by the enforcement of prohibitory measures by one State when license is given to fishermen of neighboring States bordering upon the same lake to prosecute their calling without interference.

It is not the purpose of the writer to criticize special restrictions which have been put upon certain methods of fishing, but the object is more to call attention to the seeming inconsistency of attempting to control the fisheries of a lake by measures so widely at variance as those that are known to exist in the legislation of the different States that are interested in the lake fisheries.

This was written in 1890. Probably the first "International Fish and Game Conference" organized on the Great Lakes to discuss the advisability of procuring uniformity of fish laws was called at New York on October 12, 1891, by Dr. G. A. MacCallum, fish commissioner of Ontario, Canada. Dr. H. M. Smith represented the United States Fish Commission. This conference appointed a committee, which met at Rochester on November 10, 1891. This international conference met again at Hamilton, Ontario, on December 9, 1891, and at the third and last meeting, held at Detroit on December 21 and 22, 1892, adopted a series of resolutions that prohibited the destruction of small fish, provided for closed areas, closed seasons, and size limits.

Another attempt to establish uniform laws was made at a conference held at Chicago in April, 1904. The Hon. C. D. Joslyn writes of this conference as follows:

The result of the meeting was that we did agree upon several recommendations to be made to the respective legislatures of the States represented. But the sad sequel of it all is that not two of the legislatures agreed to nor followed these recommendations. Some States followed some part of them, other States followed some other part, but there was no uniformity and no unanimity. So that we are just where we started—nothing accomplished.

Discussing the question of uniform laws, Mr. Joslyn concludes:

In the circumstances, then, nothing like uniformity can be attained. Still, if the fish of our Great Lakes are not to be entirely destroyed, if they are to be propagated, planted, and protected in these waters so as to produce the best or even beneficial results, it must be done in a uniform manner, under uniform laws, uniformly enforced. It is too plain for argument that this uniformity can only be brought about on our side of the Lakes by our National Government. So forcibly have these things come home to us who live on the Lakes that at the meeting in Chicago, which I have mentioned, it seemed to be the unanimous opinion of those present that the entire matter of propagating, planting, and protecting fish in our inland seas should be put in the hands of the Federal Government. This is certainly and surely the only means of saving the food fishes which naturally thrive in them for those who are to come after us.

I may say that as a result of the Chicago meeting the States of Minnesota and Wisconsin alone lived up to their agreement to pass resolutions ceding their control of the commercial fishery of the Great Lakes to the National Government.

A large portion of the meeting of the American Fisheries Society held at Erie, Pa., July 23–25, 1907, was devoted to a discussion of Federal supervision of commercial fisheries, and several resolutions were passed. So great was the determination at this time to succeed that in several instances bills ceding State rights to the National Government were drawn and actually submitted to the legislatures for enactment, but as usual without avail.

Since 1907 many papers and discussions have been devoted to the subject of uniform laws and Federal control, that finally culminated in another series of international conferences—those referred to above and those held at Columbus, Ohio, and at Lansing, Mich. These efforts to reach agreement on fisheries legislation, in which the bureau always took a very active part, are commendable, but in view of the history of past efforts I hope I may be pardoned for my pessimistic outlook as to their success.

In conclusion permit me to enumerate the salient features of the progress on the Great Lakes during the past two years in which the bureau took an active part:

1. The unification and coordination of the various scientific agencies interested in the study of the limnology of Lake Erie, which led to the most extensive and most complete limnological survey ever undertaken on the Great Lakes.

2. The inauguration of a new system of collecting fishery statistics adequate for use by biologists in studies of depletion.

3. The establishment of a closer contact with fishermen's organizations and State officials than has existed in the past. Instead of simply drawing up a set of general regulations for the Great Lakes and dumping this on the unsuspecting public to have it take its own course, an attempt is being made to bring the various factions together to recommend specific and uniform laws for each lake. We have, for instance, taken a very important part in the codification of the commercial fishing laws of Michigan.

4. The bureau has advanced materially the cause of uniform regulations on the Great Lakes by conducting the international conferences and by drawing up the program and most of the proposed regulations.

5. The bureau has inaugurated a series of studies on the fisheries, entirely new for the Great Lakes: (*a*) A study of effect of gear on the fish population; (*b*) a study of fish populations rather than of individual species; (*c*) a study of fluctuations in the fisheries; and (*d*) a study of the unknown or partly known life histories of nine important market species.

6. Last, but not least, the bureau's Great Lakes scientific staff has grown from two individuals to seven, and its funds have been substantially increased. I believe the bureau may well point with pride to its achievements and progress on the Great Lakes during the past two years.

INVESTIGATION OF WISCONSIN LAKES

Prof. Chancey Juday, *University of Wisconsin*

It gives me great pleasure to have this opportunity of expressing our great appreciation of the kindly and helpful cooperation that we have had from the Bureau of Fisheries for more than 20 years. The assistance that we have received during this time has enabled us to accomplish much more than we otherwise should have been able to do. Thus, the bureau deserves a fair share of credit for whatever success we have attained in our lake investigations.

For convenience of discussion, the Wisconsin lake studies may be separated into three fairly definite periods. The first period includes the work done on the dissolved gases of our lakes between the years 1905 and 1910. This investigation dealt chiefly with quantitative studies of dissolved oxygen and carbon dioxide and their relations to the various biological processes which take place in lakes. This field of research has become so well known in recent years that further discussion of our work need not be given here.

The second period extended from 1911 to 1917. During this interval quantitative and chemical studies of the plankton of the lakes situated in the vicinity of Madison were made. From 1911 to 1914, inclusive, these studies were limited to the net plankton. Various tests showed that large amounts of material were lost through the meshes of the nets, and a method for the recovery of this lost material was finally developed and used from 1915 to 1917. It was found that about 98 per cent of the organisms that were lost through the meshes of the net could be recovered by means of a powerful centrifuge. A machine intended for clarifying oils and varnishes was used for this purpose, and tests showed that it was not only able to remove practically all of the larger organisms, but also from 40 to 75 per cent of the bacteria.

In order to obtain enough material for chemical analyses it was necessary to filter considerable quantities of water (from 3,000 to 15,000 liters, depending upon the size of the crop) in order to obtain enough net plankton. Smaller amounts of water were used for the centrifuge catches—from 1,000 to 2,000 liters.

Most of the observations were made on Lake Mendota, but for purposes of comparison Lakes Monona and Waubesa were included in the investigation. On Lake Mendota catches were made once or twice a week during the open season and about once a month during the winter, so that the observations are numerous enough to give a good picture of the variations in the plankton crop throughout the year. The observations on the other two lakes were made at much less frequent intervals.

One can not determine the annual production of plankton material, because plankton production continues through the year. There is no definite harvest season as on land, so that one has to be content with ascertaining what we have called the "standing crop."

The average standing crop of total plankton in Lake Mendota yielded 214 pounds of dry organic matter per acre, while the live weight of this would be approximately ten times as much, or a little over 1 ton per acre. While it is impossible to estimate the annual turnover in this stock of plankton, it may be pointed out that such large organisms as Daphnias will produce an average of two broods per month from May to September, or ten broods during this time. Most of the other plankton constituents will reproduce much faster, but

a tenfold overturn during the year would mean an annual production of 1 ton of dry organic matter per acre in Lake Mendota, or 10 tons of living organic matter per acre.

Observations have been made on lakes ranging in depth from 35 feet to 225 feet, and the total yield of plankton per unit of surface was found to be substantially the same. This seems to show that the production of plankton is independent of the depth within these limits; in other words, it seems to be a function of the area and not of the depth.

The chemical analysis showed that plankton is a very nutritious material, since 44.5 per cent of the dry organic matter consisted of crude protein and 7.5 per cent of fat; that is, these two food substances made up 52 per cent of the dry organic matter.

Plankton is made up of food (phytoplankton) and feeders (zooplankton). Rough determinations showed that the phytoplankton weighed from 12 to 18 times as much as the zooplankton, which ought to insure a sufficient supply of food material for the latter. In spite of the excess of food material, however, it requires a good deal of work on the part of the feeders to obtain a sufficient amount of food. A Daphnia, for example, must strain about 13 cc. of water to obtain her own weight of organic matter from the plankton, which means that she must extract all of the food from 60,000 times her own weight of water in order to secure her own weight of food.

The zooplankton forms constitute the chief connecting links between the phytoplankton and the fish population. While some of the small fish feed directly upon the algae, the great majority take their plankton food in the form of animals that feed more or less extensively upon the phytoplankton.

In addition to the plankton crop, a quantitative survey of the large aquatic plants has been made in two of our lakes, namely, Mendota and Green. In Mendota these large plants extend down to a depth of 23 feet, and over this area the yield averages a little more than 1 ton of dry material per acre. About 75 per cent of the dry weight of these plants consists of organic matter, or approximately 1,500 pounds per acre. The plant zone covers a little less than one-third of the total area of the lake, so that these large aquatics furnish annually about 350 pounds of dry organic matter to the lake as a whole. This quantity is less than twice as large as the average standing crop of plankton—namely, 214 pounds per acre. Since the plankton has an annual turnover estimated at 10 times or more, its contribution of organic matter to the lake will be much greater than that of the large aquatics. The large aquatics also yield a much smaller percentage of crude protein than the plankton.

In Green Lake the yield of large aquatics was only 1,600 pounds per acre in the 0–27 foot zone, or about 300 pounds of organic matter per acre when the area of the entire lake is taken into account.

The bottom fauna is another important source of food for fish, and quantitative studies of this population have been made on both Mendota and Green Lakes. On the basis of dry weight, the 0–1 m. zone in Green Lake yielded only 8 pounds per acre, dry weight, while the 20–40 m. zone gave 30 pounds and the entire lake bottom an average of 20 pounds per acre.

Mendota yielded an average of 53 pounds of dry material in its bottom organisms per acre, or a little more than two and a half times as much as Green Lake.

Alm has given some results that show the relation between the production of bottom animals and the production of fish in a number of lakes in Sweden. The yield of bottom animals in these lakes ranges from a minimum of about 1 pound per acre to a maximum of 215 pounds per acre, while the fish production in these same lakes ranges from about 1 pound to 100 pounds per acre.

Pearse estimated that about half a million perch are caught each year in Mendota. Since most of these perch are rather small in size, their weight may be estimated at 100,000, which would mean a yield of about 10 pounds per acre. This does not include the yield of the other food fish found in the lake.

Lake Nipigon is reported as having yielded an average of 1 pound of fish per acre for a period of 4 years. The yield of Attersee in Austria is reported as a little more than 2 pounds per acre, and the Alpine lakes of Bavaria and Switzerland yield from 7 to 13 pounds of fish per acre, while the lakes of southern Italy are reported as producing from 18 to 27 pounds of fish per acre annually.

The third period extended from 1918 to the present time. The earlier years of this period were used largely for developing apparatus and methods for more extended investigations. The large centrifuge was so valuable in the plankton work that a portable one that could be operated in the field became a necessity. Accordingly our mechanician, Mr. Foerst, took up this problem and soon developed a small, continuous-acting centrifuge, which can readily be taken into the field and operated wherever an electric current is available.

As the work progressed, also, it became desirable to extend the chemical work and take into consideration the problem of the fertility of the aquatic soil. The productivity of a lake is dependent upon the fertility of the water, which constitutes the chief aquatic soil, just as the productivity of the land is dependent upon the fertility of its soil. Aquatic plants, like land plants, are dependent upon the presence in an available form of certain nutritive substances for their growth and reproduction. This is true particularly of the free-floating phytoplankton organisms. In accordance with this plan various chemical and biological studies of the lakes of southeastern Wisconsin were made between 1918 and 1924. These investigations gave a general idea of the chemical and biological conditions that obtain in lakes that possess relatively hard waters. The lakes of northeastern Wisconsin were known to have much softer waters than those of the southeastern part of the State, and a study of the northeastern group of lakes was decided upon after a preliminary survey in 1924.

A temporary laboratory was established at the State forestry headquarters on Trout Lake in 1925, and it has been maintained here each summer since then. Three new buildings were used as laboratories in the summer of 1928, and they will now be occupied solely for this purpose as long as the investigation is continued in that region. The laboratory is situated in the midst of a lake district; something over 300 lakes have been visited so far, and most of them are within 25 or 30 miles of the laboratory. Many more are within easy reach.

These northeastern lakes occupy basins in a glacial outwash plain for the most part, and they differ very greatly in character. There is a wide range in sizes and depths, and a number of them are isolated, possessing neither an inlet nor an outlet. The great majority of them are connected with streams. Most of them are situated in the Wisconsin River Basin, but some of them drain into Lake Superior and others into Lake Michigan.

We may now turn to a brief consideration of some of the chemical and biological results that have thus far been obtained. Most of the chemical work is now done on the fresh samples of waters, but it is impossible to do certain phases of this work in the field. For studies of this character samples of 2½ to 6 liters of water have been evaporated at a temperature of about 70° C. on kerosene stoves, and the residues thus obtained are used for such analyses. The amount of residue obtained in this manner ranges from about 9 mg. per liter in the softest lakes to 107 mg. per liter in the hardest one. Such small samples make it necessary to use microchemical methods for the residue determinations, and such methods have now been worked out for most of these determinations. Samples of 5 to 10 milligrams each are used for most of the analyses.

In the various field observations are included such physical items as temperature, transparency, color, and conductivity of the water, and such chemical items as hydrogen-ion concentration, free and fixed carbon dioxide, dissolved oxygen, free ammonia, nitrite, nitrate, and organic nitrogen, soluble and organic phosphorus, silicia, and chlorides. So far the biological phases of the work have included chemical and quantitative studies of the plankton and quantitative studies of the bottom fauna.

The hydrogen-ion concentration in the various bodies of water varies from pH 5.4 to pH 9.1. The highest acidity has been found in the bog-lake waters, and it is undoubtedly due in part to the presence of organic acids. The most alkaline waters have been obtained from lakes that possess a fair amount of fixed carbon dioxide in solution and where there is a relatively large crop of growing phytoplankton.

In most of these lakes carbon dioxide is present in three different forms, namely, free, half-bound, and fixed. In those that have alkaline waters, or a pH greater than 7, there is little carbon dioxide, because it has been used up by the aquatic plants in their photosynthetic processes. In general, the free carbon dioxide ranged from zero up to 4 or 5 parts per million. The algæ not only make use of the free carbon dioxide in photosynthesis, but they

are also able to make use of about three-quarters of the half-bound carbon dioxide; the amount of half-bound carbon dioxide in turn is proportional to the quantity of fixed, so that the total available supply of carbon dioxide is dependent upon the amount of carbonates in solution. In the lakes having the softest water, such as Crystal, for example, there is very little available carbon dioxide, and this seems to set a limit to the production of plankton. These lakes invariably produce smaller crops of plankton than those that have harder waters, though some of the shallow soft-water lakes (those that do not exceed 10 or 15 feet in depth) are able to support large growths. This appears to be due to the fact that the waters of these lakes are kept in circulation at all depths by the wind, so that the water is freely exposed to the air, where a new supply of carbon dioxide may be obtained; and it also comes in contact with the bottom, where decomposition is actively in progress.

The quantitative studies of the various nitrogen compounds show that there is no free ammonia or nitrite nitrogen in the surface waters, or only a trace. The amount of organic nitrogen is dependent upon the quantity of plankton chiefly, and it ranges from 0.13 to 1.36 parts per million. The nitrate nitrogen ranges from 0.008 to 0.050 parts per million, or 8 to 50 milligrams per cubic meter of water. On the whole, it may be said that the supply of nitrogen is abundant enough to supply the demands of the phytoplankton.

In recent papers Atkins has suggested that phosphorus is the limiting factor in the growth of phytoplankton in fresh waters as well as in the ocean. This element is present in relatively small amounts in both fresh and salt water, so that a large growth of phytoplankton will soon exhaust the available supply in the upper water. Furthermore, when these organisms die and sink into the lower water they carry with them this supply of phosphorus, leaving the upper water in a depleted condition. This limits the further growth of the phytoplankton until the circulation of the water brings this phosphorus to the surface once more.

In a few of the northeastern lakes no soluble phosphorus has been found in the surface water at certain times in the summer, but in the vast majority of them the quantity of phosphorus remains fairly uniform throughout the summer. The usual amount of soluble phosphorus ranges from 3 to 6 milligrams per cubic meter of water. In general, therefore, these results do not tend to confirm Atkins's theory. The quantity of organic phosphorus varies from 7 to 85 milligrams per cubic meter, so that the amount is generally from two to several times as large as that of the soluble phosphorus.

A micro-chemical method is now being developed for a quantitative study of the potassium in the samples of residues that have been collected.

In addition to the various mineral substances that are dissolved in the water, there are relatively large amounts of organic matter either in solution in the water or in the form of colloidal particles, which are small enough to pass through the pores of a Berkefeld filter. The amount of this dissolved organic material varies from a minimum of about 3 milligrams to a maximum of 40 milligrams per liter of water. The largest amounts have been found in those lakes that have very high-colored waters. The color of these waters is due to peaty extractives brought in by drainage water from marshes. In general, this dissolved organic matter is six times as large as the organic matter in the plankton. In the case of the colored waters it is still larger.

It has been suggested that this dissolved organic matter may be used as a source of nutriment by aquatic organisms, even by fish, and this question is still being debated. While this material appears to be present in considerable amounts at first thought, yet it represents only 3 to 40 parts of organic matter per million parts of water, so that it constitutes a pretty dilute nutrient solution, to say the least.

As already indicated, the lakes with very soft water usually yielded the smallest amounts of plankton. The amount of dry organic matter in the surface water ranged from a minimum of 0.2 to a maximum of 7 milligrams per liter in the various lakes that have been visited so far.

A quantitative study of the bottom population of these lakes was begun during the summer of 1928, but the results have not been tabulated. The general impression gained from a casual scanning of the records leads me to think that they support a much smaller bottom population than Mendota and Green Lakes.

The diverse chemical and biological characters shown by these lakes naturally raises a question as to their effect on the fish population of these bodies of water. Just how do the variations in the quantity of plankton and of bottom organisms affect the rate of growth of the fish? Does the rate of growth in the very soft-water lakes differ from that of the same species in the lakes that have much harder waters? Are certain species of fish better adapted to certain types of these lakes than to other types? Several questions of this sort may be raised in this connection. Through the cooperation of the Bureau of Fisheries an effort is being made to answer some of these questions. During the past two years several thousand fish have been obtained from different lakes; these specimens have been measured, weighed, and samples of their scales taken for age determinations. Only a small number of these scales have been studied so far, so that no definite answer to any of the above questions has yet been obtained.

SOME FRESH-WATER SPORT-FISHERY PROBLEMS

SETH E. GORDON, *conservation director, Izaak Walton League of America*

It may seem somewhat presumptuous on my part to appear before a conference of scientists and commercial fishermen, but, as the fond parent is supposed to have said to his young heir, "this hurts me more than it does you." However, I am sure that such intermingling of interests will be good for all of us, but I do wish that our national president of the Izaak Walton League, Dr. Henry Baldwin Ward, who is favorably known to many of those present, could be here to help me ward off possible brickbats.

By way of introduction, let me warn you that I was born with a rod and line in my fingers, and as a lad of five was nicknamed after the most inveterate angler in all that section of Pennsylvania. While I have acquired some of his skill as an angler, I must confess that I did not inherit his prowess as a purveyor of choice fishing experiences. Probably, as some would have us believe, you scientists have so stunted the growth of the present-day fishes that none of my catches ever induce descriptive eloquence and fanciful tales.

Seriously speaking, however, allow me to say that I have given fresh-water angling problems considerable study from the standpoint of an angler as well as a Walton League official in an effort to help, as President-elect Hoover so well put it at one of our recent national conventions, "reduce the time between bites." In fact, I have not worried so much about the time between bites as I have about the chances for the man or boy of average means to fish at all. Our best angling, just like our hunting, I regret to say, is rapidly drifting into the hands of wealthy business and professional men of America.

I have had an unusual opportunity during the past two and a half years to learn something about the fresh-water sport-fishery problems of America. As the conservation director of the Izaak Walton League I made many personal contacts, with a few inspections here and there at 3 or 5 dollars each for nonresident angling licenses, in practically every State of the Union. I have had as a fact-finding machine over 3,000 chapters of the Izaak Walton League as well as the aid and cooperation of State and Federal officials. As a result, I feel that I know something of the wishes of the anglers as well as the needs of the fish.

The development of the country, our rapidly increasing population, short-sighted lumbering operations followed by uncontrolled forest fires, drainage of lakes and marshes, good roads and automobiles, and an appalling increase in the number of anglers have decidedly intensified the fresh-water angling problems. But industrial and municipal wastes, commonly referred to as pollution, have done more to destroy many thousands of miles of good angling streams and lakes than all other factors.

There are those who claim that lumbering and forest fires have not materially injured our fishing. Hundreds of specific examples can be cited to prove that denuded hillsides have hastened rapid run-off, thereby inducing erosion and increasing the intensity of floods, filling up the pools and eddies and destroying the sheltering places for the fish, injuring or destroying the aquatic and vegetable life in the streams, and, generally speaking, ruining streams for fishing purposes.

About 80,000,000 acres of former forest lands are now a barren waste. This, added to the 250,000,000 acres that were cleared for farm lands, has naturally

made material inroads upon our natural water-storage reservoirs. During the year 1927 the United States suffered a loss of $33,000,000 from 158,000 separate forest fires, which will adversely affect every stream shed touched until a new forest floor and new growth take the place of the materials that furnished food for the flames. Fortunately the people of America, and especially the anglers, are becoming fire wise and are doing more every year to prevent and stop them.

Good forest cover stabilizes the flow of streams and thereby benefits fishing waters. I am much encouraged to find many of the old uneconomic farm lands of the East being abandoned, and especially to note the rapidity with which these lands are being reclaimed by the forest. It was a mistake ever to clear many of these thin-soil farms, and nature is merely reclaiming her own. Fishing streams will benefit thereby.

Drainage of lakes and marshes to increase tillable lands has reduced the fishing waters of America by many thousands of acres. It likewise has seriously affected stream run-off and interfered with nature's scheme of things.

There is no use discussing the good-roads phase of this problem, or the enormous increase in the number of anglers. The only sensible thing to do is to bring back natural conditions so far as possible and increase the natural production, as well as the output of our hatcheries and rearing ponds, to meet the demands.

With reference to the pollution situation as it affects the fresh-water angler, I am sorry to say that in many cases those responsible for the pollution are apathetic, and so long as public opinion does not demand clean streams and lakes we will be compelled to put up with the present unsavory situation. There is no weapon so effective as crystallized public opinion, and there is only one way to get it—by organized effort.

The Izaak Walton League, at the suggestion of the Conference on Outdoor Recreation, undertook the first nation-wide clean-streams campaign. The entire machinery of the league has been used most effectively in the development of a proper program. Other organizations have joined with us, and the progress to date is very encouraging. Unfortunately many of our coworkers imagine that conditions that have developed over a period of 50 or 100 years should be remedied almost overnight. However, as a result of our unceasing campaign we have aroused the American people on this question as they have never before been awakened, with the result that progressive municipalities everywhere are taking steps to clean up their municipal wastes. Likewise many of the large industrial groups are giving serious study to their problems, some of them having employed experts to supervise their activities in that direction.

The one dark blot on the horizon is the stubborn attitude of certain industries; namely, that they have been allowed to build their plants on certain streams, which are now so badly polluted that there is no hope of ever restoring them to their original purity or any semblance of it, so why spend any of their money trying to remove their filth from said waters? The heads of certain industries have repeatedly said that in their opinion certain rivers should forever be left as open sewers to carry away the filth of municipalities and industries. Can you imagine such shortsightedness in progressive America? Especially when we compare conditions here with the far better stream conditions that prevail in many of the older nations of the world? The Izaak Walton League takes the position that no stream or lake should ever be used permanently as a public sewer. We also take the position that industries should help themselves in the elimination of their wastes rather than expect the public to find ways and means for doing so and at the same time pay the bill.

The Walton League's clean-streams campaign has been predicated upon the need for clean streams and lakes to furnish pure waters for both industrial and domestic use and for recreational purposes, thereby safeguarding public health. We believe that if our waters are clean enough to supply these needs the fish life and other aquatic resources of our waters will likewise be safeguarded.

We have noticed that wherever the league has a chapter progress toward clean stream conditions has been much more rapid, and that where a group of chapters join hands to clean up a watershed, or all the waters within a State, still better results are obtained. We propose to keep up our fight unceasingly until the streams and lakes of America are cleansed of these wastes, which we would not think of throwing into our streets or into our neighbors' back lots. The handwriting is on the wall—the American people are demand-

ing pure waters instead of open sewers, and they'll get them if they do their part. This conference very well realizes what this will mean to the fresh-water angler.

There are now approximately 10,000,000 anglers in America. Twenty-five years ago there were scarcely a million of them. In the meantime fishing laws and regulations, licenses, police forces, and other restrictive machinery have been built up and constantly increased, until to-day in many States a disciple of Izaak Walton needs a disciple of Blackstone constantly at his side to keep him out of the toils of the law. This probably explains why there are so many anglers in the ranks of the legal profession—their friends encourage them to go along to do double duty.

Seriously speaking, however, let me say that in most States scores of laws and regulations have been depended upon to perpetuate the fish supply, but in most instances the results hoped for have not been realized, and the only effect has been to confuse the angler who desires to obey the law. What we need in every State is simple, uniform laws, local regulations only where absolutely essential, a discontinuance of unsportsmanlike practices, and ample finances to operate the fisheries activities in a businesslike manner.

Laws alone will never save our fresh-water angling, but we must have such regulations as necessary to prevent overfishing, to stop irresponsible people from taking fish at all seasons of the year, and especially to prevent as much disturbance to the fish as possible during the spawning period. In a number of States I regret to find that the fish laws have been badly neglected. They permit the taking of bass right off the nest, but they would not think of allowing anyone to shoot a wild turkey, or quail, or ruffed grouse while hatching and rearing her young. Probably you scientists can convince the public that fish are not fit for food at that time. People abhor the idea of eating the flesh of a setting hen but keep right on catching spawning fish for both sport and food.

A good way to protect spawning fish is to set aside the general spawning period as a close season and to allow no fishing at all during that time, or by establishing fish refuges, or closing sections of streams as breeding waters. We are establishing refuges for game; why not do so for fish? This might also offer a suggestion to the commercial fishermen present.

The use of all kinds of destructive devices in inland lakes and streams where sport fishing is of prime importance should be discontinued just as quickly as possible. Many of these devices are permitted under the guise of taking food fish for home or local consumption only, whereas the privilege is in reality almost universally abused, and game fish are taken in wholesale quantities. It is hard enough to keep up the supply of fresh-water fishes to meet the demands of the hook-and-line fishermen without catering to pleas made by unreliable commercial fishermen who all too often would gut and loot the waters of every fish they can catch lest some competitor beat them to it.

In many sections this attitude has built up an almost insurmountable barrier between the anglers and the commercial fishermen, even though farsighted leaders on both sides are doing their best to play fair. There is no reason why common ground, upon which both interests can easily agree, should not be found, but it will take careful handling to avoid still more serious clashes.

One of the species about which there has been considerable controversy is the black bass; another is the steelhead trout, and sometimes the striped bass. But is there any real reason for these misunderstandings? Quite frequently one hears the United States Bureau of Fisheries condemned because it does not take a more agressive stand against the commercial fishermen in the matter of these species. At other times the local State administration is charged with favoring the commercial fishermen, and I suppose there are times when both the United States bureau and the States are criticized by the commercial fishermen for apparently favoring the anglers. And so the battle rages.

Some of the species I have named should be given far more protection than is now given them. The black bass, for example, is worth far more as a sport fish than he can ever be as a market fish. Many of my friends say the same thing about the striped bass and the steelhead. But in view of the enormous demands upon our inland streams and lakes there is certainly no longer any excuse for permitting the sale of black bass over the market stall. They are worth their weight in gold, because any section of the country that can boast good bass fishing will reap a harvest of tourist gold far greater than the dead fish would ever bring.

The commercial fishermen yield to this opinion most stubbornly, and by so doing have made it difficult for themselves as well as for the anglers. In an effort to stop the sale of black bass, Senator Hawes, of Missouri, and the Izaak Walton League succeeded in securing the enactment of an act in 1926 to stop the sale or shipment of black bass. That law, however, is effective only in States that have laws making it illegal to take bass for commercial purposes or to offer or ship them for sale. We have succeeded in scaling down the list to eleven States, and in one of these the State's own bass may not be sold, but those shipped in by her neighbors may be sold. The league is now making a concerted effort to have these remaining 11 States stop the sale of black bass. Let me suggest to the representatives of commercial fishing interests here present that they can win thousands of friends by openly espousing such legislation. The principal offenders are Maryland, Virginia, North Carolina, Florida and Tennessee. Four of these permit commercial fishing for bass in restricted areas only, but so long as there is a legal market anywhere you may bank on it that thousands of pounds of illegally caught bass will be sold. The way to stop it is to close all the markets, then give the United States Bureau of Fisheries some funds for the enforcement of the Hawes bass law. The bureau must take a firm stand and help stop the sale of these gamy denizens of our fresh waters.

In the matter of the steelhead and the striped bass on the west coast, the commercial fishermen would do well to meet the anglers more than half way, because their problems in many instances are mutual and should be fought out together rather than to be fighting each other.

I have pointed out a few of the problems confronting the fresh-water sport fisheries, but the one I have reserved for consideration at the close of my paper is, in my opinion, at the very foundation of the entire problem—an ample staff of scientifically trained men to direct the work and plenty of funds, so that definite long-term programs may be developed. There has been entirely too much groping in the dark, and we are all highly pleased to find the leaders developing more backbone and less wishbone. You scientists here present can well be proud of your profession. You are paving the way and are elevating your work to a standard never before attained. But we need more scientifically trained men in this field. Please do not misunderstand me, as I have nothing but admiration for the dozens of successful men in the field of fish culture who never had an opportunity to secure special training to fit them for this specialized field. In their day it was not to be had, but the boys and girls of to-day can and will get this training if properly encouraged, because more of the institutions of higher learning are offering such courses every year. This augurs well for the future.

There are, however, some disturbing factors, as a recent survey of the fish-cultural work being done by the States developed some rather startling information. I obtained reports from all the States except Maine and Mississippi, and found that the 46 States reporting are operating 414 stations, of which 308 are State-owned, the balance being cooperative projects.

Five of these 46 States have no hatcheries—Arkansas, Georgia, South Carolina, Tennessee, and Virginia. I understand that at least three of these have plants under way. Thirteen of the States have 10 or more stations. California heads the list with 24, Wisconsin 21, Washington 20, New York 19, Michigan 17, Colorado, Montana, Ohio, and Oregon 14 each, Minnesota 13, Maryland 12, and Idaho and Illinois 10 each. There are 16 States with from 5 to 9 stations, among the outstanding ones being Connecticut, Massachusetts, and Pennsylvania. The total investment, exclusive of Connecticut, Missouri, New York, Washington, and West Virginia, is $9,357,000. The grand total is likely around $12,000,000. Pennsylvania heads the list with $1,505,000 invested; Colorado is next with $1,000,000; Ohio, $900,000; Michigan, $625,000; Wisconsin, $550,000; and Minnesota and California, $500,000 each. Of the 41 States with hatcheries, 37 of them spend $1,875,000 annually for operation and salaries, practically all derived from angling licenses, some of it from commercial fishermen. California, New York, Oklahoma, and Oregon failed to give definite figures in response to that question, but I am of the opinion that by adding the amount expended by these four States the total average expenditure for operation of the State hatcheries will be about $2,500,000, or probably slightly more. This, then, gives us a total investment of about $12,000,000 in property by the States and an annual operating cost of about $2,500,000.

The total output is rather difficult to compute because of the fact that quite a number of States still distribute fish in the fry stage in large quantity; but, according to the best figures I was able to compile, the annual distribution figures for 42 States are 84,000,000 trout, 24,000,000 bass, 48,000,000 pike and pickerel, 280,000,000 crappie, bream, blue gills, and perch, and 14,000,000 catfish, sunfish, and other species. Several States also report enormous quantities of commercial species which were not included in the table given.

These figures could easily be broken down to the average cost per fish as well as the average contribution by each of the 10,000,000 anglers, but in view of the large amount expended in law enforcement and various other ways to increase and perpetuate the fish supply for the angler I did not deem it wise to do so. I am sorry to say also that in a number of States the anglers are still parasites; either part or all of the cost of their fish work is paid by the hunters. But this situation is rapidly correcting itself; the fishermen realize as never before that if they want more and bigger fish placed in their streams and lakes they must supply ample funds to do the work. Several States hope to enact fishing-license laws this winter, and a number of others are planning to increase their fees. I have always found the angler willing to pay his full share if he is satisfied that it will be put to good use; he is not a piker.

I hope to obtain figures covering the remaining States as well as like figures covering the work of the Bureau of Fisheries, after which an interesting composite can readily be prepared; but the final point I want to make pertains to personnel, always an interesting study.

The 46 States gave me data on the number of superintendents and other employees, their salaries, perquisites, etc., and I was surprised to learn that in only 9 States are the hatchery employees under civil service. The most astounding development, however, was the amount of the salaries being paid, and the more I studied them the more I became convinced that young men can not be induced to secure special college or university training to make fish-culture and allied subjects their vocation until two outstanding requisites are given attention—namely, the remuneration and the tenure of office.

The 319 superintendents receive salaries running all the way from $1,200 to $3,000 per annum, but in the great majority of States about $1,500 to $1,800 is the top salary. Practically all of these employees are furnished homes, heat, and light, but until the inducements at the top average better than they now do the prospects are not what they should be.

The 171 assistant superintendents receive $840 to $2,000 per annum, but for most of these employees about $1,500 is the top salary. Many of the assistant superintendents as well as some of the other employees are supplied with homes; but the average pay is too low to attract and hold the best men (unless they are just plain cranks, as many of us are) when industries offer much better wages with living conditions more to the liking of an average family.

I admit that I have sketched a pessimistic picture, but I have great hope for the future. With 3,000 chapters of the Izaak Walton League and hundreds of other like organizations scattered throughout America, many of them operating rearing stations, nursery ponds, brood ponds, and some of them hatcheries, we are bringing to your aid an organized support never before possible. I am proud to say that about 250 of these Walton League chapters are spending their time and money helping to rear millions of baby fish to a size that gives them a fighting chance when released in the streams and lakes, and thereafter they take an intense interest in seeing to it that those fish receive the protection they deserve. However, more thorough stream study is necessary before these fish are planted. We expect several hundred additional chapters to take up this rearing work during 1929, but this activity needs the aid of more trained workers.

In conclusion, let me say that every one of the league chapters is 100 per cent behind the Hoover nursery-pond plan, the 5-year program sponsored by Congressman White, the irrigation-ditch screen studies, the effort to bring back the commercial fisheries of the Atlantic, the Pacific, the Gulf, and the Great Lakes, and especially are they anxious to have more technically trained, well-compensated men supplied with ample funds to do the work.

So long as we can keep an ardent angler-conservationist in the White House we should all have ample reason to rejoice. Better days are coming.

PROGRESS IN FISH-CULTURAL INVESTIGATIONS

Dr. H. S. Davis, *in charge of aquicultural investigations*

At the present time the investigations dealing with fish-cultural problems center, for the most part, around the activities of the Fairport biological station and the experimental trout hatchery at Pittsford, Vt. This arrangement provides for work with both trout and warm-water fishes, but within rather narrow fields, since only a few species of fish can be handled at either station. Owing to limited facilities, it has been necessary to concentrate on the largemouth bass at Fairport and on brook trout at Pittsford hatchery, although other species have not been neglected entirely.

Probably the most important feature of the work at the Pittsford station during the past two years has been the feeding experiments conducted under the immediate supervision of Mr. Lord. For the first time since these experiments were inaugurated I feel we are in position to discuss with some degree of confidence the relative value of the different foods we have been testing.

Recently we have heard a great deal about an ideal trout food, the idea being that once we can determine the requirements of trout in terms of proteins, carbohydrates, fats, vitamins, salts, and other food accessories we shall be in position to compound a food that will give maximum results. Even were it possible to determine the optimum amounts of these foods, which I seriously doubt, it does not follow that we would then be in position to supply our fish with the ideal food.

It should be remembered that the fish-culturist does not have an unlimited number of foods at his disposal from which he can pick and choose. On the contrary, he is bound hand and foot by considerations of cost and supply. Any food to be used successfully in feeding trout, or other fish for that matter, must be obtainable at a reasonable cost, and there must be an adequate supply available at all times. Of course, I realize there are some exceptions to this statement where certain cheap foods are available locally, but as a general rule the fish-culturist is limited strictly to a few food products that are obtainable in quantity at a low cost.

Available trout foods can be divided readily into two groups. In the first group we have the fresh meats of various kinds, including the liver, heart, and lungs of cattle, sheep, and pigs. Possibly fresh fish of low grade should be included in this group, although this product is not at present universally available. In the other group we have the various dried products, including meat meals, fish meals, clam meal, shrimp meal, dried milk, and cereals.

There is at least one point on which all trout culturists are so well agreed that it may be said to be axiomatic, and that is that trout will not do well for any length of time unless some fresh meat is included in the diet. This fact, which has been demonstrated time and time again, has recently led McCay to postulate a hypothetical factor H which is present in all fresh meats but not in dried products, at least to any extent.

It has been the universal experience of fish-culturists that trout can be reared successfully on a straight meat diet, although there has been much difference of opinion as to which meat gives the best results. Our experiments with both brook and rainbow fingerlings indicates that, in general, a mixed-meat diet is preferable to one composed of only a single meat product. For instance, a mixture of heart and liver will give better results in the long run than either heart or liver alone. Also, our experiments have shown conclusively that the same organ from different animals will often give very different results. Of course this is only what one would naturally expect, and we are not surprised, therefore, that beef liver and beef heart give much better results than do the livers and heart of pigs or sheep. In fact, when using a straight meat diet we have obtained our best results with a mixture composed of equal parts of beef heart and beef liver.

Undoubtedly the results obtained with various meat products are dependent on both their chemical and physical structure, and I am convinced that the latter is not to be ignored by any means. This, I believe, largely explains the superiority of beef heart over beef liver as a food for advanced fry and very young fingerlings. The heart can be ground into very fine particles, which are readily swallowed by the young fish, while the liver forms a thick, mush-like material not so readily separable into discrete particles. Furthermore, a considerable percentage of the ground liver is readily soluble in water, and there is, therefore, a considerable loss in food value from this source.

As the fish increase in size they are able to ingest larger particles, and the food is devoured more quickly. Consequently, there is less loss from solution, and liver now will produce a faster growth than heart, although there is usually a higher mortality. By combining heart and liver to form a mixed diet we find that the growth is practically as great as with liver alone, while the mortality is comparable with that of fish fed on a straight heart diet.

But, unfortunately for the fish-culturists, all fresh meats have advanced in price materially during recent years, and the use of beef liver in promoting growth and combating anemia has placed this product outside the pale of commercial fish culture. Consequently, in our feeding tests during the last year or two we have concentrated on the problem of finding suitable substitutes for part of the fresh meat. These substitutes include various dried products of animal origin, wheat middlings, soybean meal, and Mexican beans. Our experiments indicate that some of these substitutes can be used successfully in combination with fresh meats, but, with one exception, none of these combinations have produced as rapid growth as a suitable combination of fresh meats alone.

As stated above, our experiments for several years consistently show that among the foods ordinarily fed to trout a mixture of beef and beef liver has no superior, and we had begun to believe that this mixture probably represented the best artificial food available. However, we have found during the past season that a mixture of beef liver and so-called "clam heads" is superior to the liver and heart mixture, both in regard to growth and mortality. Not only did the fish thrive better on this diet than on any other, but it was found that "clam heads" in combination with pig liver or sheep plucks gave nearly as good results as with beef liver. Unfortunately, the supply of "clam heads" is limited, but it is hoped that some similar product can be found, which will be available in quantity.

Other animal meals that have given satisfactory results are shrimp meal, haddock meal, cod-liver meal, and dried milk. While these meals did not produce such a rapid growth as "clam heads," the experimental lots showed a fair growth and low mortality.

In connection with the use of animal meals in the diet, the problem arises as to the proper proportions of fresh meat and dried meals. In other words, how much of the fresh meat can be replaced by a substitute without affecting the fish adversely? Of course, from the standpoint of cost alone the smaller the percentage of fresh meat in the diet the better, but, obviously, there are limits to the extent to which meat can be replaced by a cheaper product. Nor is it purely a matter of nutrition, since the physical consistency of the mixture must be taken into consideration. This factor has an important bearing on the readiness with which the food is eaten and also on the amount of nutriment lost in solution.

In preparing the mixtures of fresh meat and meals it has been found best, in most cases, first to mix the dry meal with water to the consistency of a thick mush and then incorporate with the ground meat. When this mush is mixed with an equal quantity of meat it forms a mixture of about the right consistency for feeding to fingerlings. Since most meals absorb from 1½ to 2 times their weight of water, such a mixture contains about 15 to 20 per cent of the substitute, based on the dry weight. It is believed that this is about the maximum percentage of dried products that can be used successfully in the diet of fingering trout, but with yearlings and older fish a considerably larger proportion may be used. In several instances we have obtained excellent results with yearlings in feeding a ration composed of only 33 per cent of fresh meat, and it is not impossible that the percentage could be reduced still further without detriment to the fish. Since most animal meals cost from 3 to 5 cents a pound and fresh meats from 10 to 20 cents it will be seen that the food bill can be more than cut in half by a liberal use of the former.

In continuing the feeding experiments next summer it is planned to devote more attention to the food of fish above fingerling size than has been possible in the past. With the constantly increasing demand for larger fish for planting purposes the problem as to the most economical food for fish 4 inches long and upwards is becoming more acute. We are also planning experimental feeding with brood fish to determine, if possible, whether the food has any influence on the quality of the eggs. In many instances there is a very

considerable loss among eggs from hatchery fish, and there is evidence that this may be due, in part at least, to improper food.

Before leaving this subject I should like to digress for a moment to point out that, in my opinion, too much emphasis has been placed on obtaining rapid growth in hatchery fish to the exclusion of other factors. Rapid growth is very desirable, but it should not be procured at the expense of the health and vigor of the fish. The Federal and State hatcheries are raising fish to be liberated in natural waters, and it is very doubtful if the fat, lazy, chunky, pot-bellied trout often seen in hatcheries are as well able to care for themselves, when suddenly thrown upon their own resources, as are fish that have not been forced to the limit. The former are like hothouse plants, and, when suddenly transferred to natural waters, we may expect about the results we obtain when plants are taken from a greenhouse and transplanted into an open field without further attention. For stocking purposes we need fish with some vim and pep, and this fact should be taken into consideration in judging the comparative value of different foods for growing trout.

Another important phase of the work at Pittsford is the experiments in selective breeding. Since there was no stock of fish at the station when it was taken over by the division of inquiry it was impossible to accomplish anything in this direction for the first two or three years, and only now have we reached the point where the work can be taken up seriously. A few fish have been mated during the past two seasons, but these can be considered only in the nature of preliminary experiments. However, this preliminary work has served to emphasize the importance of breeding experiments, and I have no doubt that the results will be of great value to practical fish culture.

With few exceptions very little attention has been paid by trout culturists to the principles of selective breeding. True, a number of growers have taken pride in building up a superior stock of brood fish, but actually the selection has not been carried on systematically for a number of generations, as must be the case if results of permanent value are to be obtained.

The most important work in this field is the experiments of Embody and Hayford at the New Jersey State hatchery. These experiments have been conducted for the purpose of increasing the resistance of fingerlings to disease, and also to develop a stock with a more rapid growth. According to a preliminary account of their work, they have been able in three generations to produce an apparent increase in resistance to disease and also a marked improvement in the rate of growth. In fact, so great was the increase in growth as a result of selection that the average size of the fingerlings on July 1 jumped from 2 to 4 inches in length. I understand that since these results were published, they have still further increased that rate of growth among the selected stock.

Our preliminary experiments at Pittsford have shown that the offspring of different parents show marked differences in the rate of growth during the first summer. They have also shown that there is a noticeable tendency for fingerlings from the same parents to grow at a uniform rate, so that there is much less variation among such fish than among fingerlings of mixed parentage.

The results of Embody and Hayford, just mentioned, were obtained by mass selection; and, in fact, this is the only type of selection that has hitherto been practiced by trout-culturists. While we are using mass selection to some extent in our own work, we are laying greatest stress on the selection of individual fish, as it is believed that this method will yield results that will more than compensate for the increased labor and expense.

During the spawning season just closed 34 pairs of brook trout were mated, and the young will be reared separately. In order to follow the parent fish from one year to the next all are tagged, and a complete record is kept of each individual fish.

One of the greatest difficulties in selective breeding is to determine correctly the relative value of fish of different parentage. In order to provide a common basis for comparison as free as possible from error, all fingerlings produced in our breeding experiments will be reared during the first season under as nearly identical conditions as possible. Since it is obvious that two of the most important factors that influence the health and growth of fingerling fish are the food and available space, it is absolutely essential that these be the same in the case of each lot. Otherwise direct comparison of the fish at the end of the season may lead to very erroneous conclusions.

Investigations carried on at the Pittsford hatchery have enabled us to clear up many obscure points in connection with two common trout diseases. These diseases are both caused by bacterial infections, which in one case affect chiefly the gills and in the other the fins.

The gill disease was first recognized at the Pittsford station, where it has annually caused serious losses virtually ever since the station was established. However, the disease is now known to occur at many hatcheries in the East and Middle West, and during the past summer I found it in fingerling salmon from Birdsview (Wash.) hatchery.

This disease is caused by slender rod-shaped bacteria, which are usually joined end to end to form long filaments. The bacteria do not penetrate the tissues but are found attached to the surface of the gills, and in the fry they may also be found on the body and fins.

The gill disease causes very serious losses among fry and fingerlings, and even older trout occasionally die from its effect. During the past summer it was found that fingerling trout may become infected from yearlings and older fish that show no trace of the disease. This indicates that the bacteria may occur in small numbers on perfectly healthy fish for an indefinite time, which, obviously, will make it very difficult if not impossible to eradicate the disease from a hatchery. Fortunately, the disease is easily controlled by the copper-sulphate treatment.

The other disease referred to above is commonly known as "fin rot" or "tail rot." "Fin rot," like most trout diseases, is also widely distributed and has been found in hatcheries from the Atlantic to the Pacific coast. As indicated by the name, the disease is caused by a bacterial infection of the fins, which begins at the outer edge and gradually extends toward the base. The bacteria follow the lymph channels to the base of the fin, whence they may invade the muscular tissues. As a result of the infection the fins may be entirely destroyed. In some cases infection may take place through the skin at some distance from the fins, forming sores filled with a white pus. Later the pus may be washed out, exposing a clean-cut cavity, which often looks as though it had been hollowed out with a knife.

Observations made during the past summer indicate that "fin rot" is much more prevalent in our hatcheries than has been realized. Apparently the organism varies greatly in virulence, and in many cases the disease occurs among fingerling trout as a comparatively mild infection, which causes a small daily mortality without assuming the proportions of an epidemic. In such cases a majority of the fish recover from the disease without suffering any serious injury, and the fins rapidly regenerate the parts that have been destroyed.

"Fin rot" can also be controlled by copper sulphate, but owing to the nature of the infection is not as susceptible to treatment as is the gill disease. Since bacteria that have gained entrance to the tissues can not be reached by the solution, the treatment has little effect on advanced cases. This necessitates several successive treatments at frequent intervals in order to stop infection during the early stages and prevent the disease from spreading to healthy fish.

The investigations in pondfish culture, started three years ago at the Fairport biological station, have been devoted primarily to work with the largemouth black bass. The constantly increasing demand for bass for stocking purposes has emphasized the importance of developing methods of propagation and rearing these fish economically. For various reasons bass culture has lagged behind trout culture until to-day the demand for bass far exceeds the available supply. When we consider that the streams and lakes suitable for bass and other warm-water fishes are many times more extensive than those adapted to trout we begin to realize the magnitude of the problem of keeping these waters properly stocked.

As everyone knows, the methods employed in pondfish culture differ radically from those used in handling trout. Unlike trout and salmon, bass and other warm-water fishes must be allowed to deposit and hatch their eggs under essentially natural conditions. This means that a much smaller number of fish can be reared per unit area of water surface, which, of course, materially increases the overhead. That, however, is offset to a considerable extent by the fact that it is not necessary to feed pondfish regularly, as in the case of trout, and the labor of caring for them is much less. In fact, I doubt if the propagation of bass as compared with trout is such an expensive proposition as many people have been led to believe. Given the proper facilities, I can see no reason why bass may not be propagated and reared both successfully and economically.

The Fairport experiments have demonstrated that bass can be reared successfully in comparatively small ponds; in fact, it is doubtful if it is advisable under ordinary conditions to use ponds over 3 to 4 acres in area. Small ponds have several advantages, not the least of which is the greater ease of control.

The best results in rearing fingerling bass have been obtained in nursery or rearing ponds. The fry as they rise from the nests are caught in a floating trap designed by Mr. Hesen and are then placed in nursery ponds, which previously have been stocked with minnows to serve as forage for the young bass. In this way we have been able to produce as high as 10,000 three-inch fingerlings to the acre without supplying any food other than that produced in the pond. It is interesting to note that in 1926, the first year of experimental work, the average production of bass was 4,400 fingerlings per acre; in 1927 it was 5,300 fingerlings per acre; while in 1928 the average production had risen to 7,100 per acre. These figures include the production in both the spawning and nursery ponds.

At the present time we are using two species of minnows for forage purposes with good results. The larger of these is the golden shiner, *Notemigonus crysoleucas*, which in some respects seems to be the better of the two; but the smaller blackhead minnow, *Pimephales promelas*, is also an excellent forage fish. The superiority of the golden shiner is probably due chiefly to its larger size. Owing to their small size the adult blackheads are readily devoured by fingerling bass before the summer is over, which makes it more difficult to maintain a stock of forage fish in the ponds than in the case of the shiner. It is possible that both species can be used in the same pond to advantage, since the blackhead minnow is primarily a bottom feeder while the golden shiner is not.

The relative value of the two species is indicated by the results of experiments in the E ponds during the past summer. These are small ponds of approximately the same area and situated side by side. Two of these ponds were stocked with golden shiner, the other two with blackhead minnows. Later, 3,900 bass fry were liberated in each pair of ponds. When drawn in the fall there were 1,507 fingerlings in the shiner ponds with a total weight of 220 ounces. On the other hand, the ponds stocked with blackheads yielded only 307 fingerling bass, weighing 116 ounces.

By the use of forage fish in bass ponds we are able to produce not only larger and better fingerlings but the percentage of survival is also much greater. In one instance there was a survival of approximately 56 per cent, and we have several examples of between 40 and 50 per cent survival.

The fertilizer experiments are an important part of the pond work and have yielded some very notable results. In 1926 a mixture of bone meal and dried sheep manure was applied to several ponds at frequent intervals during the spring and early summer, but the results were not conclusive. During the following summer (1927) superphosphate was used instead of bone meal, and in every instance the fertilized ponds showed a much greater production than the unfertilized. In several instances where the results were directly comparable the weight of the bass produced in fertilized ponds was from two to four times that from the unfertilized.

In addition to largemouth bass, several other species have been propagated in the Fairport ponds. Good results have been obtained with bluegill sunfish, black and white crappie, and smallmouth bass. Attempts to propagate the white bass, *Roccus chrysops*, and yellow bass, *Morone interrupta*, have so far been unsuccessful; and it is believed that it will be necessary to obtain more information regarding the spawning habits of these fish before we can hope to make any progress in this direction. We are also attempting to improve the stock of bass and sunfish by keeping the best fingerlings for brood fish and also by obtaining superior strains of wild fish.

In connection with the pond investigations, Mr. Wiebe has been conducting a series of experiments on the influence of various fertilizers on plankton production. These experiments, which have been carried on in small concrete pools, have shown that soybean meal produces more plankton than any of the other fertilizers used. In view of the favorable showing made by this meal it is planned to try it out on a more extensive scale in the bass ponds next season.

During the past season we have been able to extend our investigations on warm-water fishes to the Upper Mississippi River wild-life refuge. Here is presented an excellent opportunity for studies on fish under natural conditions, which will supplement and amplify the work carried on at Fairport.

In order that we may have a comprehensive idea of conditions in the refuge, Mr. Surber has been engaged during the past summer in making an inventory of the ponds and sloughs available for aquicultural investigations. With the aid of a houseboat he has been able to cover the most important parts of the refuge and collect data that will be invaluable in developing a systematic program for future work. Although the survey was necessarily superficial, Mr. Surber has been able to determine the general nature of most of the ponds and sloughs in the area covered and to classify them on the basis of their probable value for fish-cultural work.

In the past there has been considerable discussion regarding the feasibility of increasing the production of desirable fish in natural waters, but so far no one has had the temerity to attempt to do this on any extensive scale. In venturing to apply the principles of practical aquiculture to such an extensive area as the bottom lands of the Upper Mississippi we are embarking on what is virtually an uncharted sea and are subjected to all the perils of an inglorious shipwreck. However, I believe the time has come for action rather than a continuation of theoretical and academic discussions, which get us nowhere in particular.

Mr. Surber's survey has shown that certain sloughs appear to offer a much more favorable opportunity for experimental work than others, and it is on this type of slough that we purpose to concentrate for the present. If we are able to make progress and solve some of the problems presented by such ponds we shall then be in position to extend our operations to more difficult fields.

While the bottom lands of the Upper Mississippi River wild-life refuge afford, in the aggregate, a large number of sloughs and ponds under Federal control, and to that extent provide exceptionally favorable conditions for such a program as we have in mind, we are, nevertheless, laboring under a very severe handicap. The periodical floods, which may occur at almost any time, present an obstacle that in many cases it will probably be impossible to overcome. Only overflowed lands are included in the refuge, and in order to obtain ponds that are not subject to occasional floods it will be necessary to go outside the limits of the reservation. However, in spite of these difficulties we are optimistic regarding the future and believe that much can be accomplished that will be of permanent value to aquiculture.

BIOLOGY AND CULTIVATION OF SHELLFISH

Dr. Paul S. Galtsoff, *In charge of oyster investigations*

Of the numerous marine organisms inhabiting the inshore waters, the oyster occupies a unique place; it grows in great abundance in nearly every harbor along the Atlantic coast and Gulf of Mexico and plays, therefore, an important rôle in the biological and chemical changes that occur in the inshore waters. To understand the rôle of the organism in the cycle of changes that take place in the sea and to discover the factors that control its propagation and growth are the aims of our investigation. From the scientific point of view it is unimportant what organism we are dealing with; it may be mollusk, sponge, or fish, but fortunately the study of the biology of the oyster opens remarkable possibilities. Working on the oyster we are dealing not only with an animal that is widely distributed and important from an economic point of view, but we have an organism that easily yields to experimental treatment, both in the laboratory and in its natural habitat. This makes it possible to apply the results of laboratory work to the field experiments and carry them out on a large scale.

For many years the method applied in the study of life in the ocean consisted in descriptions of the organisms—grouping them in so-called ecological communities and giving rather incomplete records of the physical and chemical conditions and changes that take place in the sea water. This purely descriptive method affords very little opportunity for analysis of the controlling factors operating in any body of water and is inadequate for the understanding of the complexity of interrelated events that determine the propagation, growth, and distribution of living matter in the ocean. In order to understand any biological phenomenon in the ocean we must possess better knowledge of the functions and requirements of a given organism and understand the operation of the limiting factors that govern its life in the sea. This requires long-continued experimental study and implies the introduction of experimental investigations into the field of oceanic biology, which up to the present time was

based on descriptive method. The procedure is more difficult than the old scheme of collecting the organisms and attempting to correlate the biological data with the few physical and chemical observations that form a traditional program of oceanographical observations, but it opens a new and promising field of research. On these general ideas we have attempted to develop the program of oyster investigation.

The first phase in our study deals with the propagation of the organism. Several ecological papers on the biology of the oyster describe rather vaguely the spawning of the oyster; they state that the discharge of sex products takes place when the temperature of the water reaches 68° or 72° F. Vivid descriptions of the spawning of oysters in tanks or floats are given by Brooks, Stafford, and Nelson. It has been known from these papers that the discharge of gonads is accompanied by the contraction of the adductor muscle and clapping of the shells. This information gives, however, little help in understanding either the mechanism of the reaction or the conditions under which it takes place. The organisms kept in natural environment are under the influence of a number of variable chemical and physical factors that can not be controlled and can easily escape our attention. The reaction should be studied in the laboratory under as far as possible controllable conditions. Three years ago I began the experiments on spawning in the laboratory at Woods Hole. The method used in this study consisted in immobilizing the oyster and connecting one of its shells to a "heart lever," which records the motion of the shell on the slow-moving kymograph. The oyster is placed in a 30-liter glass tank filled with sea water; the temperature, oxygen content, and pH are kept constant.

There is a distinct difference between the spawning of the female and male oysters. In the female the reaction consists in rhythmical contractions of the adductor muscle and discharge of the eggs, while in the male the discharge of sperm is not accompanied by any muscular activity, and consequently the spawning of the male can not be recorded with any self-registering device.

Both males and females can be induced to spawn by placing them in warm water; in the males the reaction takes place easier than in the females, and exposure of an hour or two at the temperature 5° or 7° C. higher than the original causes a discharge of eggs. It is more difficult to induce spawning in the female; out of many experiments I succeeded only once, when the female was kept for five hours at a temperature of 26.5° C. As a result of many experiments it has been found that in both females and males the spawning reaction can be provoked by the addition to the surrounding water of small amounts of eggs or sperm.

There is, however, a distinct difference in the reactions of the two sexes. We shall consider first the reaction of the female. When sperm is added to the water in which the female is kept there occurs a latent period, which varies from seven minutes to half an hour; then the adductor muscle begins to contract rhythmically, each contraction being followed by a brief period of relaxation. The kymograph records show the difference in the ordinary contraction of the muscle and those occurring during spawning. The reaction lasts from 15 to 40 minutes and varies considerably in intensity and number of contractions. The contraction may be accompanied by the discharge of eggs, but in many instances the muscular activity caused by the sperm is not followed by the shedding of the sex products; apparently the two phenomena are independent of each other. When there is a discharge of eggs the reaction is more complex. When the female is ready to spawn it opens its shell wide; then the edges of the mantle on each side of the organism come in contact with each other, nearly shutting off the pallial cavity from the outside, except one small opening where the edges of the mantle are slightly gaping. When the mantle contracts the eggs are ejected through the opening; then the edges of the mantle come apart, the pallial cavity opens, and in a few seconds begins to close again, the process repeating itself until the spawning is over. When there is no discharge of eggs the mantle cavity remains open all the time.

One of the typical features of the reaction is the rhythmical contraction of the adductor muscle, which makes it possible to detect it on the kymograph record even when there is no discharge of eggs. The intensity and duration of the reaction, as well as the duration of the latent period, are independent of the amount of sperm. The reaction complies well to a "none or all" principle—the sperm or the active principle of the sperm is the agent that releases

the mechanism, but it exerts no effect on the intensity of the reaction. There is a certain minimum dose of sperm capable of producing the reaction. In our experiments it was determined as being equal to 0.03 c. c. of sperm suspension made of 1 gr. of gonad tissue in 100 c. c. of sea water and added to the 30 liters of sea water.

After the reaction is over the oyster fails to respond to subsequent additions of sperm and for 3 or 4 days remains unaffected by it. The period of insusceptibility presents considerable complication in the experimental study and requires numerous additional experiments. It certainly should be considered very carefully in interpreting the negative results of the experiments.

There is some evidence in my experiments that the female oyster can be rendered insusceptible toward sperm by adding several small doses, each of which is too small to cause the reaction. Although the total amount of sperm is larger than the minimum effective dose, the organism does not react.

The response of the male to the addition of eggs is quite different. When the male is ripe the addition of eggs to the water causes an immediate discharge of sperm, which is carried off with the outgoing current. There is no increase in muscular activity. The reaction can be repeated many times, until the oyster has discharged all its sperm.

In both males and females temperature is one of the important factors. After a number of experiments convincing evidence has been obtained showing that the spawning reaction in the female does not occur at a temperature below 20° C. The most conclusive proof was obtained last summer with one oyster that, being subjected to temperatures of 16° and 18° C., failed to spawn but reacted two hours later when the temperature of the water was raised to 20.6°. In these experiments the same suspension of sperm was used. The spawning of the male oyster was observed only at a temperature of 22° C. or above.

On the basis of these experiments it is possible to visualize how spawning takes place in nature. It is very probable that when the temperature reaches 22° (or above) some of the males discharge sperm, which induces the spawning reaction in the females. The eggs discharged by the female instantly cause the ejection of sperm by other males and the process spreads all over the bed. If the gonad development is very light, and consequently the concentration of sperm in the water is low, the females may become unsusceptible and spawning will be delayed for at least four days. One of the practical deductions than can be made is that successful spawning should be expected only on densely populated oyster beds. It also seems possible, when the temperature of the water is 22° C. or higher, to start spawning of oysters in the shallow harbor by opening a few of them and pumping the suspension of sperm or eggs over the bed. Experimental evidence also shows that oysters may spawn several times during the 4 or 6 weeks of the spawning season. One oyster has spawned in laboratory tanks five times, each time discharging a certain amount of eggs.

Study of the spawning reaction shows that in addition to temperature the discharge of sex products is controlled also by a chemical agent discharged with them. It is very interesting to determine the nature of it, but unfortunately the problem is not an easy one. It has been found that the active principle is very unstable, being destroyed by heating for 5 minutes at 44° C.; its action is specific, the sperm of foreign species having no effect on the organism. Ultrafiltration experiments made last summer convinced me that the active principle is not soluble in sea water and does not pass through collodion membranes. It is interesting to note that the presence in sea water of these substances, which can not be detected by ordinary methods of analysis, plays an important rôle in the propagation of marine organisms and controls their spawning. It is very likely that what has been found for the oyster is true also for other forms.

Next to spawning the question of feeding and respiration is one of great importance in understanding the requirements of the organism. During the past year the study of the mechanical activity of the oyster gills has been finished and published. This work was undertaken in order to solve certain problems that arose in connection with sanitary control. It was of particular interest to find out how temperature affects the activity of the gills; it has been found that the majority of oysters cease feeding at 7° C. No oysters were found that were able to feed at a temperature of 5° C. or below. This fact was taken into consideration by the committee on sanitary control of oyster beds, which regarded that the slowing down of the activity of the oyster at this

temperature affords an additional factor of safety. It was, therefore, possible to permit, during the cold weather, the marketing of oysters taken from bottoms that were slightly and indirectly affected by pollution. The introduction of this clause in the resolution of the committee probably saved many thousands of acres of oyster bottoms to the oyster industry.

The method of study of the functions of the gills was fully described in my paper. It suffices to mention here that it consists in collecting and measuring the amount of water passed through the gills or in determining the rate of flow of water in the cylindrical tubing that was introduced and made fast in the gill cavity. The results of these experiments give us the exact filtering capacity of the average oyster and throw some light on the conditions under which the oyster lives in nature.

Knowing the average rate of flow of water for an adult oyster at a given temperature, it was possible to estimate the amount of water used by the oysters on a crowded reef. In certain shallow bays in Texas, where the number of oysters on the reef is known, there is not water enough to supply the demand, and the same water is filtered several times by the oysters growing on the reef. This is apparently one of the direct causes of the very poor quality of their meats.

The development of the gonads and the quantity of spawn in the ripe oyster apparently depend on the food consumed. It has been noticed that when the first half of the summer is cold the gonad development in oysters on Long Island Sound is light. Suppose that for a certain summer the average temperature for May and June was, for instance, 5° below normal. Knowing the average rate of flow at a given range of temperature and the average duration of feeding, we can compute the deficiency in the amount of water filtered. Prytherch applied this method to his work and found, under these conditions, that the "average" oyster in Long Island Sound would have filtered about 200 liters less than it would at the normal temperature for these months. These examples show us that it is possible to develop this method further and obtain reliable data regarding the feeding of oysters under different conditions and to apply them to an analysis of the ecological conditions in a given body of water.

Other physiological work on oysters consisted in a study of the rate of oxygen consumption, which, as it has been determined, varies in proportion to oxygen tension. It has been found, also, that the ciliary activity is not affected by the changes in O_2 tension, and that there was no difference in the rate of metabolism in green and normal oysters.

The study of greening of the oyster has been continued. Experimental evidence so far obtained tends to show that the accumulation of copper in the tissues of the oyster is not due to the direct absorption of copper but is probably caused by changes in catalytic reactions, resulting in the accumulation of copper normally present in sea water. Our present work consists in the analysis of conditions responsible for the production of greening in the oyster.

Other psysiological work on chemical sensitivity and factors controlling the activity of the adductor muscle were carried on by Doctor Hopkins. The study of the biology of the oyster larvæ was continued chiefly by Mr. Prytherch, who paid particular attention to the factors controlling the setting of the larvæ. As a result of this investigation he was able to work out a method of prediction of spawning and setting. I wish to mention that his estimations for Connecticut waters issued one month in advance proved correct.

It has been noticed in our previous work that the appearance and distribution of oyster larvæ has some relation to tidal movements of the water. In certain localities it was very difficult to obtain the larvæ, although later the setting was very heavy. There arises a question whether the behavior of the larvæ is influenced by the movement of water, and this problem was taken up by Doctor Luce, who carried out a number of experiments at Milford. Owing to certain technical difficulties no definite results were obtained, but it is expected to carry out these experiments and find out how the velocity of tidal currents affects the behavior of the larvæ. We found that in tidal rivers it is impossible to obtain oyster larvæ at the high flood tide, while at low slack water they might be abundant. The study of the biology of the larvæ is essential for the development of a better method of catching spat and for the increase in the production of seed oysters.

In New England waters the industry is handicapped by the fluctuations and inconstancy in seed production, and it is our belief that this can be overcome by adoption of a better method of spat collection. The development of an efficient method of spat collection was one of the principal problems in which several investigators were engaged during the last three years. The experiments were carried out at Onset, Wareham, Milford, and Great South Bay, and always with successful results. We are in position now to recommend this method to the industry. It consists in using 3 feet by 1 foot wire bags of 1½-inch mesh filled with oyster shells. The bags are placed on the bottom and, if necessary, can be stacked in various ways. Three years of experimenting with various collectors proved that wire bags are superior and that by using this method the production of seed oysters per given area can be increased from 8 to 40 times. The increase is due, first, to the fact that three dimensions of the setting zone are utilized instead of only two, as in the ordinary method. Second, after they have caught some spat the bags can be removed and new ones placed instead. In this manner the concentration of spat on the shells can be regulated. Third, the bags can be placed on bottoms unsuitable for ordinary planting. As an example I want to mention the results of last year's experiments in Onset, in which a few bags were placed in Sunset Bay. This bay is regarded by the oystermen who tried to plant shells there as absolutely unsuitable for catching seed and was left unutilized. We succeeded in obtaining in this location around 5,000 spat to a bushel, which should be regarded as a fair commercial set. Local oystermen are becoming more interested in this method, and last year they planted several thousand bags and obtained good results.

For a successful application of the method it is necessary to know the prevailing currents in the bay, to determine the level of the setting zone, and to know the distribution and number of spawners. The determination of the setting zone can be made by examining the piles, rocks, or other objects within the tidal zone or on the bottom. The necessity of having a large number of spawners in the immediate vicinity of the planted bottoms is frequently overlooked by the oystermen, who waste their money in planting shells with no assurance of heavy spawning in the bay.

Experiments in oyster culture were extended to Georgia, and last summer Doctor Weatherby made a great many observations at the temporary laboratory at Doboy Island. Oyster beds in Georgia suffer from both depletion and overcrowding. Because of the heavy setting in the tidal zone the reefs produce poor coon oysters, while the few good reefs with single oysters are almost completely depleted. As an illustration of the wasteful methods employed in this State I want to mention the following observations. Cluster oysters taken from the reefs and brought to the shucking houses always contain a considerable percentage of small oysters. At one of the houses last spring I found that the oysters delivered to the shuckers were unusually poor, and the clusters were formed by very small oysters; counting their number in clusters I found that to one adult oyster, the meat of which was taken by the shucker, 44 were destroyed and wasted because they were too small to be opened. This represents not only waste of the oyster but a tremendous waste of labor and expense in carrying, steaming, and disposing of shells. An interesting fact observed by Weatherby is that setting around Doboy Island takes place on the bottom of the tidal river. This fact is of importance because it opens better possibilities for oyster culture in Georgia waters.

Surveys of oyster bottoms were carried out in North Carolina by Seiwell, in the Potomac River by Doctor Luce, and on the Pacific coast by myself and McMillin. From a biological point of view the situation on the Pacific coast is of great interest.

There are three species of oysters found on the Pacific coast—the native Olympia oyster, the imported Japanese, and the eastern oyster. The purpose of the survey was to determine what species of oyster affords the best opportunities to the industry. We begin our discussion with Olympian oysters. This small oyster is being cultivated in various bays in the southern end of Puget Sound; Olympia can be regarded as the center of the industry. The oyster is cultivated on tidal flats that are exposed at low water. Owing to the very high range of tide, varying from 12 to 30 feet, a large area of the tidal flats become exposed at each low water. In order to make cultivation possible there has been developed an elaborate system of dike construction at

different levels of the tidal slope. In some of the bays there are four or five levels of diked areas. The shape and size of each area varies, depending on local conditions, but usually each dike surrounds about one acre of bottom. The dikes are built of concrete and are from 3 to 7 feet high and about 1 foot wide. The bottom inside the dike is graded and reinforced with gravel. Improved first-class oyster bottom represents an investment of $2,000 or $3,000. The dikes require constant attention. The greatest trouble is caused by the decapod crustacean Hypogebia, which bores under the dike and causes a wash out and leakage under it. To protect the dikes the oystermen built a wall around it, using 6-foot boards of creosoted lumber.

It is amazing that while so much effort and money are spent on the improvement of bottom very little attention is paid to the oyster itself, and virtually nothing is known regarding the exact time of its ripening, spawning, and setting. The latter, as a rule, is very light, three or five spat to a shell being regarded as a good catch. We know that the Olympian oyster is hermaphroditic and that it grows very slowly. From a commercial point of view it is a valuable oyster, and the demand for it is greater than the supply. It is cultivated on privately owned farms, which, in spite of great expense in building the dikes and high taxation, bring a good return. According to the information I received during my trip, a small farm of 24 acres brings a gross income of about $5,000 a year. Oyster farming in these waters can be extended considerably without any danger of overproduction, and I believe that a biological study of the Olympian oyster will be of great help to the industry.

Attempts to import the eastern oyster have been made many times, and although we found a few young eastern oysters in Willapa and other harbors in the State of Washington, it is generally believed that this species can not be propagated on a commercial scale.

The importation of the Japanese oyster presents an interesting problem. Several thousand bushels of Japanese seed oysters are imported annually and are planted in Samish Bay. The oyster grows fast but does not spawn. Scattered Japanese oysters can be found in other sections of Puget Sound, and a few young Japanese oysters were obtained in Nasel River. Oysters in Samish Bay were examined in October; they were full of spawn but apparently were unable to discharge their sex products. It appears feasible, by studying the spawning reaction of this oyster, to find a method whereby they can be induced to spawn, and it is planned to carry out these experiments in the near future.

The importation of Japanese seed is undesirable because of the danger of introducing the Japanese drill, which might destroy valuable Olympian grounds. From a commercial point of view the Japanese oyster is inferior to the native oyster, which should be protected from enemies. Dealing with this problem our efforts should be applied in two directions—first, in outlining practical measures of control and the inspection of imported seed with the view to prevent the admission of drills and other enemies, and second, in studying the conditions under which the Japanese oyster can be propagated in Puget Sound. Examinations of bottoms in Puget Sound, Gray's Harbor, and Willapa Bay show that all three oysters may spawn and set in these waters. It is a problem of biological research to find a method for their propagation and cultivation than can be applicable to commercial oyster farming in a given locality. I believe that the solution lies in a study of local conditions and in the intelligent application of the results obtained with eastern oysters on the Atlantic coast

ARTIFICIAL CULTURE OF GLOCHIDIA OF FRESH-WATER MUSSELS

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The experiments that have led to the working out of an artificial nutrient solution in which to rear the glochidia of certain of the fresh-water mussels have been very largely physiological in character. In these experiments mussel larvæ have been carried through their parasitic stage, thus replacing the developmental period ordinarily passed in cysts on the gills of fishes by one in glass vessels under controlled laboratory conditions. It has been established for many years that cells and bits of tissue can be made to grow outside of the body of the animal from which they were taken if kept in fluids in which

the proper chemical balance of salts and nutrient substances is maintained. The development of a method for the handling of the glochidia (which, after all, are little more than bits of embryonic tissue) has therefore been a study of the conditions under which the glochidia live while parasitic on the fish by tissue-culture methods.

The first problem to which a definite answer was obtained was that concerning the status of the glochidium while encysted on the fish gill. It has been stated by some observers that the glochidium during its parasitic stage received little or no food from the host fish, but that the fish acted merely as a carrier, providing at once both a distributional agent by means of which the juvenile mussels were scattered upstream as well as downstream, thus assuring the mussel species a safe distribution against the river current, and a safe place for metamorphosis from larval glochidium to juvenile mussel. That is, the relation between the fish and the glochidium was a sort of passive parasitism. From analyses of fish blood and sera, salt solutions having the proper osmotic pressure, hydrogen-ion concentration, and salt balance were prepared; and in these solutions glochidia of various species of mussels were carried under suitable conditions of oxygenation. The glochidia lived well in these solutions, although the solutions themselves had no nutritive value for periods longer than those required for transformation had these same glochidia been encysted on fish gills. There was, however, little or no development in these salt solutions, and the glochidia always died after a longer or shorter time following the disintegration of the large muscle that holds the two valves together.

Solutions of balanced salts, to which dextrose was added in amounts comparable to those found in fish blood and sera, were next tried. Glochidia lived in these solutions even longer than in the solutions containing salts only; but the final result was always the same, namely, little development and finally death after the disintegration of the muscle.

As it is well known that the glochidia when closing on the gills of fishes include small bits of the host tissue between the valves of the glochidial shells, and that these bits of tissue subsequently disappear, it was thought that the glochidia might require the initial stimulus of some protein or enzyme from the fish, and that, once started, the development of the glochidium might continue in the salt solutions; that is, that, after all, the glochidia might carry sufficient food material in themselves for metamorphosis if properly stimulated at the start. Accordingly, glochidia were closed on bits of fish gill, liver, muscle, and blood clots and then transferred to the salt solutions described above. The glochidia in these series showed fine starts toward development but soon failed and then died, just as those carried in the salt solutions alone had done. From these experiments it was evident that the glochidia do not carry sufficient food material in themselves to complete metamorphosis, and that the fish does supply some substance or substances which are essential for more than mere starting stimulation. No matter how valuable to the mussel species the protection and distribution factors are, the fish host adds some other required part in the successful life cycle of these mussel species.

As the result of tests and analyses of the protein constituents of the fish blood and sera, it seemed likely that the substance or substances lacking in the salt-dextrose mixtures were to be found in the protein portion of the serum from the host fish. New series of solutions were compounded, and when the proper amounts of amino-acid mixtures were added to the salt-dextrose preparations, solutions were perfected in which the glochidia made complete development and transformation into juvenile mussels without any contact whatever with the host fish. The juvenile mussels reared in this way were found to be active and vigorous and, in fact, were subject to a lower mortality than juveniles raised on the fish.

The experiments that were successful demonstrated that the mussel glochidia are true parasites, obtaining a definite nitrogenous food supply from their hosts; and also proved that the artificial media that supplied these needed substances were just as successful as the fish as far as the development of the glochidia was concerned. The next problem, then, was to devise means to supply these nutrient fluids to large enough numbers of the glochidia to make the method commercially profitable. As soon as large numbers of glochidia were taken, however, additional problems were encountered. First there were bac-

teria, for these nutrient solutions were found to be excellent media for the bacteria that the glochidia carried with them from the river water; in fact, the bacteria rapidly overwhelmed the glochidia in such cultures. Then there were protozoans to be killed. It is well known that the glochidia, before leaving the marsupia of the mother clam, are attacked by several species of Protozoa, and one of these was found to thrive in the nutrient solutions. Besides, there were still other problems concerning the viability of the glochidia themselves and the condition of the glochidia at different seasons of the year. A method has been perfected which meets these various difficulties and admits of the handling of large numbers of glochidia, but the work is being continued with a view of simplifying and perfecting this method still further so that increasingly larger numbers of glochidia can be handled successfully.